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To the Editors of the Medical and Physical Journal.

GENTLEMEN,

IT probably is as well to anticipate the surprise that naturally would attend the publication of a Letter, in Answer to an Hypothesis made known at least four years ago, and which might have been so easily noticed in a late publication, (An Inquiry into the Nature and Action of Cancer, by the Author, last July). The fact is, strange as it may appear, the Author never knew such an hypothesis was in being till some time after the publication of his Inquiry, although he had made no inconsiderable search after works on Cancer. It is indeed matter of great surprise, that an Hypothesis of such notoriety, coming from an author of such celebrity as Dr. Joseph Adams, and addressed to the first physiologists of the age, should neither have drawn an attempt at refutation on the one hand, nor claimed the public devoirs of proselytes on the other. A baneful indifference too long has enveloped the subject. The Author is ashamed that an appearance of neglect should have attached to him, and hastens to make all the reparation in his power.

March 5, 1806.

A LETTER TO DOCTOR JOSEPH ADAMS, ON THE HYDATID EXISTENCE OF CANCER.

SIR,

SINCE every hypothesis can only be respectable, as far as it is supported by general experience and facts, I am confident you will not be inimical to an inquiry so governed, touching your opinion of the hydatid existence of cancer,

cancer.* And since the importance of the subject (upon which you have displayed so much classical ingenuity,) must be more or less interesting to every one, a freedom of investigation is allowed, and which, in such an instance, may be fully enjoyed, without the slightest dereliction of personal respect. I am confident your researches have aimed to establish the real nature of the disease, and not merely to build up a splendid hypothesis; under this impression, therefore, I shall not hesitate without further preface, entering upon the merits of your conclusions, which I am confident you would have every one do, before he adopted them.

I frankly confess, Sir, that however I may admire the novelty and ability of your hypothesis, considerable discordancy would appear to arise in its application: it would seem not altogether to be drawn *simply* in regular progression from what *really is:* but on the contrary, a *solitary appearance* (the contraction of the fatty cells, which is in no way constant, because the diseased structure, in which such cells are contained, is not always present in scirrhus) has given rise *(apparently)* to a false presumption in the first instance, which is attended by a train of ingeniously framed observations, as its consequences and support.

In way of prelude to the hydatid existence of cancer, you have prefixed, "An account of the dissection of a man that died of a suppression of urine, produced by a collection of hydatids between the neck of the bladder and rectum, by Dr. John Hunter, F. R. S. &c." wherein is established the separate animal existence of the hydatis lymphatica. You then proceed, in a letter to Dr. Baillie, to explain the simplicity of the life of the hydatid, by transcribing a part of "The Morbid Anatomy." "Life may be attached to the most simple form of organization. In proof of this, hydatids have been found in the brains of sheep, exactly resembling those of the human liver, and which have been seen to move; and therefore are certainly known to be animalcules: The hydatids, indeed, of the human liver have not, as far as I know, been found to move, when taken out of the body and put into warm water; were this to have happened, no difficulty would remain."

* Vide, The Doctor's Observations on the Cancerous Breast, consisting chiefly of original correspondence between the Author, and Dr. Baillie, Mr. Cline, Dr. Babington, Mr. Abernethy, Dr. Stokes, &c.

main." (Vide Morbid Anatomy, 2d edition, p. 225.) And upon a parity of reasoning which you draw from the contractile power of the coats of the hydatis lymphatica, you endeavour to rest the separate existence of the cells, often observed in the fat of cancerous structures. " No more (you say) will be required of you, than to prove the same properties (alluding to the contractile power of the hydatid coats) unconnected with elasticity in the fatty part of the cancerous breast." The demonstration of which, you reduce in the following manner.

" Immediately after the operation, take the amputated part, and cut it in a transverse, or indeed, in any direction, and wherever you discover this fatty appearance, you will see the surface at first smooth under your knife; in an instant after, you will find a papillary appearance all over the yellow green surface. Each of these papillæ you will find to be part of the contents of a capsule, the contraction of which has produced this conical figure. If the amputated part has been exposed long to the cold, or thrown into water, and left there for several hours, a section of it no longer exhibits this appearance, nor is it possible, in this stage, to distinguish at first sight, what I call carcinomatous hydatids from common fat. By this, it appears, that the yellow green transparency of the fat, and the contractile power of the enclosing tunics, could only arise from life, and the degree of heat with which that life was attended."

Now, even admitting that the contractile power of these cells is clearly demonstrated by this, their *separate animal* existence is by no means made out to authorize the classical term you have given of *hydatis carcinomatosa.* I must beg leave here to remark, Sir, that your hypothesis falls into the *separate existence* of these cells so instantaneously, that one is compleatly taken by surprise; and the only resource left one, is firmly to persist in calling out for proof. Here, we certainly want that part of Dr. Baillie's letter answered, where he hints at the *necessity* of a just and absolute distinction, between a *separate animal existence* and the *common life of* a part.

" Respecting the propriety of the term cancerous hydatid, (replies Dr. Baillie) I have only to remark, that if you can prove the cyst in a schirrous structure to possess a life peculiar or proper to themselves, like the hydatids found in the liver, there would seem to be no impropriety in the term. But if they possess merely a living principle, similar to that of the ordinary diseased structures in an

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animal body, I should think it better for you to adhere to the name of cyst."* On this point therefore, Sir, I must beg to remain at issue, till the *separate animal existence* is better established in cancerous structure, than merely by the solitary "papillary appearance" you have noticed.

In supposing these cells to be hydatids, we are obliged to commit a violence to the evidence of our senses; because we cannot forget that the breast of a woman is a *bed* of fat, and therefore we are by no means surprised to find small cells in the cancerous breast containing diseased fat; it is what we naturally look for: nor is the alteration by any means more than what we rationally can account for: no stretch of faculty is required to answer the obliteration of the lateral communications: Do we not see it every day in common inflammation? The sides of the cells are thickened. Do we not see this in every instance of abscess? I cannot therefore imagine why we should lose sight of original structure, or of known simple diseased actions, when we go into an inquiry of more complicated alterations; and why, instead of thus progressively following up disease, we should entirely wash out all remembrance of natural actions and original structures, and set about a new creation. In the altered membranous structure in cancer, (when it is present) where is there the marked characteristic of independent existence, with regard to the connecting cellular and adipose membrane? If the morbid change has not extended to the whole membranous structure of the breast, cannot one trace the progress of the alteration from the *diseased fat* in *intimate connexion* to where the line of difference is imperceptibly lost in *healthy structure*? Or, vice versa, cannot one commence with the *healthy structure*, and by progressive connexion

* The evidence necessary for a separate existence would appear not altogether to be sufficiently marked by the distinction as it here stands: Common life, and "proper life." Strictly speaking, there cannot be *two* distinct sorts of life. The different *degrees* of life, no doubt, are endless. Variously modified, and combined, it may be; yet, however, *life* can only be *life* after all. The common life of a part is its "proper life," and therefore unquestionably "peculiar" to that part. The life of a bone is as peculiar and proper to it, as *that* of the most irritable muscle: The *degree* only of the self-same principle constituting the *difference*. To prove therefore, a separate animal existence in these cells, it is not sufficient to say, that they possess a higher degree of life, than what is commonly given to morbid structures. The evidence necessary for such a conclusion is, that they should have a *distinct* chain of functions, evidently arising out of a *separate* and *peculiar œconomy* of their own.

nexion be led into the *morbid alteration?* In fact, is there not such an intimate blending and similitude between the two structures, tha the line of difference between the healthy and the diseased is perfectly indefinite? or, is there that difference between the *two*, that any one can with ease immediately point out the distinction, and say, this is the morbid structure; — here begins the hydatid mass; — this could never have been *simple adipose?* No, on the contrary, the whole weight of the distinction hangs upon a hair. This structure, we are told, cannot be distinguished in appearance (some time after its amputation) from *common fat*; and it is only by minute examination, and by cautiously picking out of the fat, that we find it less intersected than *healthy* adipose, and that the cells in which it is contained are more distinct, and their sides more dense than those of common cellular structure. Upon *this difference* rests the proof of the animalcular existence in a carcinomatous breast. And all that can be drawn in support of a *separate animal œconomy*, is the *indistinct* evidence of a *questionable* fact; viz. "the papillary appearance in recently amputated cancerous fat." Touching, therefore, the independent existence of the cells in cancerous fat, (though vessels may be obliterated, and many changes wrought which never existed in health) if such be admitted, then, in my mind, every adipose cell in the body is likewise an hydatid.

From the particular notice you take of the green transparency of the cancerous fat, observable just after amputation, (and which it loses if long exposed to cold, or if it is soaked many hours in water, when it can no longer be distinguished from common fat) you undoubtedly, Sir, seem to attach a peculiarity exclusively derived from the circumstances attendant on the life of the part. For, speaking of this appearance on the one hand, and the contractile power of the fatty tunics on the other, you say, they "could only arise from life, and the degree of heat with which that life was attended." Such an observation could only be made with the view of attaching a peculiarity — of proving some exclusive property to the part: without some such intention, the conclusion would never have been made. I cannot see that the green transparency of such fat has any thing to do with the life of the part, or any property arising out of it as a *necessary principle*. The *common degree* of heat attendant on life is fully adequate to render any fatty substance less compact, (whether in or out of the body) than "when it has been

 long

long exposed to cold;" and we know fats, when in a liquid state, have a greenish cast: there cannot therefore, be any extraordinary inference drawn from such an appearance in a mass of fat which has long been exposed to disease, and where so many foreign aids might concur in giving it that appearance. Such, for example, as a diffusion of serum in the cells from some past increased action, and which had not afterwards been wholly taken up.

To account for the alteration in the appearance of this diseased fat after any long exposure either to air or water, we must first of all consider, that the *common* deprivation of heat may contribute in a great measure towards such a change. Or, the coloring matter of such diseased fat, by exposure, or by being suffered to remain many hours in water, might either be washed out on the one hand, or bleached by the action of oxygen on the other. Such changes are daily effected on variously discolored fatty and waxy substances, and where the agency of life is out of the question. However, Sir, supposing "the yellow green transparency," in recently amputated cancerous fat, wholly to rest even on the life of the part, or, directly upon the heat arising from such life, no peculiarity is proved by it: nor can the conical figure you have discovered on the surface of a section of such fat *(alone)* attach a peculiarity to the part.* Here are appearances, and they depend upon the life of the part. What then? No one I believe ever doubted, or will doubt, but what cancerous structures possess a certain degree of *life*, in common with other parts. So that admitting these appearances to originate in the fullest sense of your meaning, *particularly* and *exclusively* from "life, and the degree of heat attendant on that life," such evidence only goes to prove, what was never doubted, viz. A living principle in cancerous structures.

I must beg your patience in returning to the "papillary appearance" you have discovered in cancerous fat, and which you attribute to the contraction of its several tunics, as that is the principal and indeed only point upon which you can rest the separate or hydatid existence of the disease. The conclusion you draw, however, is not founded *directly* on the fact itself, but on *inference* only; and then the rest of the hypothesis is made up by extracts from the life and death of the common hydatid, (which, by the bye, you take for granted is the same in carcinoma ingeniously adapted to the several appearances afforded by the

* "A peculiarity' of course is here used, in allusion to an animalcular œconomy.

the progress of cancer.* You say, the contraction of the hydatis lymphatica proves its separate existence; and then you add, "if I prove this same power in the cells of cancerous fat, I then establish the same thing, i. e. the separate existence of these cells." But permit me to ask, is contraction of their coats alone, the only proof of the separate existence of hydatids?† Would this have been sufficient to maintain their separate animal existence, supposing (instead of being found wholly detached and floating in a fluid) they had been in intimate connection with the surrounding parts, and blended with the common connecting membrane. What gave the idea of separate animal existence to hydatids in the first instance? Not the contraction of their coats; *that* was the confirmation only. It was the perfect independency of these bodies from the surrounding parts; found, as they were, floating in fluid, in which they had been generated, and in which they had multiplied, which first suggested the idea of an independent animal economy of their own: the contraction of their coats was only the common evidence of their life, which subsequent evidence, combining with these primary circumstances, placed their separate existence beyond dispute. In short, Sir, *contraction* of a part alone, is by no means adequate to prove a *separate* existence; and I appeal to yourself, whether the cells of cancerous fat are in any way proved to be unconnected, or otherwise independent, in exception from the surrounding parts, of the common circulating medium. I am free, therefore, to confess, that what you term carcinomatous hydatids, appear to me nothing more than common adipose cells, altered by disease; and even admitting that they possess a property of contraction, which they never possessed in a healthy state; I have only to go one short step farther, and say, that they have acquired that from disease, which they never possessed in health.

Permit me, Sir, to refer you to the fifth letter in your publication, which is from Mr. Cline. It is in allusion to one (I suppose) you addressed to that gentleman, but

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* In the hydatis lymphatica, the evidence of a regular growth and decay is afforded: From the minuteness of the smallest pin's head to a diameter of an inch and a half, and so on, these bodies can be distinctly traced, till they waste to an isinglass substance: In carcinoma, all this is left to be imagined.

† Since the contractile power of fibres rests upon the most simple possession of life; the principle of irritability; no peculiarity can possibly attach to the contraction of a part alone.

which does not appear in your publication. I refer you, Sir, to this letter, because it would appear, that you had been making some intimate inquiries about encysted tumors in way of support to your opinions on cancer. Mr. Cline informs you, "that he had lately taken out a small tumor from the breast, which was without the smallest appearance of cavity," but that he "has also met with cavities in such tumors, containing a greenish yellow gelatinous substance in distinct cells;" and that in other instances similar cells have contained "different fluids, sometimes like serum, and in others of a dark bloody appearance." But so far as he had hitherto observed, they did not give the idea of being living hydatids; that is, (as he emphatically remarks) a perfectly circumscribed membrane, without any communicating vessels from the surrounding parts." This is the absolute characteristic of hydatid existence: the *body must be perfectly circumscribed* and *detached.* Therefore the analogy you draw, Sir, in letter vi. however ingeniously applied, certainly does not bear you out. "It has never yet been disputed (you say) that hydatids may be filled with a bloody fluid, as well as with lymph: the case communicated by Watson, in the Philosophical Transactions, was a cluster of hydatids, some of which were filled with lymph, and others with a bloody fluid. They were attached to a "spongy substance, answering the purpose of a placenta." Now, if these were hydatids, I cannot see in what respect they differ from those cysts, which are often found in cancerous breasts, filled with the same fluids, and attached to a spongy substance, which I call the fungus."*

Now, Sir, this cluster of bags was called hydatids, not from their *own independent existence,* but from an assumption of evidence from their strong resemblance to bodies which have been found perfectly detached, and independent of any connecting medium with the surrounding parts. "Hydatids are not a part of the animal in which they are found,

* Some degree of irregularity would seem to exist here. We are led from *small cells,* containing diseased fat, to *cysts,* which are often found in cancerous breasts, containing variously coloured fluids. Such cysts are out of the question, entirely from our present purpose. In an *indirect point* of view, no one can doubt an hydatid existence in a cancerous breast, as a *chance production.* But *now,* we are talking of the little cells, or tunics, observable in cancerous fat (and which, by the bye, are so indistinctly marked, as scarcely to be known from *common fat*) as the *direct origin,* or rather, primum mobile, of the disease.

found, any more than the worms in the intestinal canal."* So that in fact such attached bags, as noticed by Watson, can only take the rank of hydatids, as it were, from courtesy. They depended upon "a spongy substance," from whence they drew their supply, and by which they were connected to the surrounding parts, so that (in this point of view) a separate animal existence in them must be out of the question. From such formations only, hydatid existence (as it is now understood), would never have been thought of, and therefore so far from their establishing the separate existence of other attached cysts, they cannot confirm their own.

In page 52, Mr. Cline informs you, the tumor he removed from the back, and which, he supposes, is the one that you allude to, weighed sixteen pounds, and was entirely adipose; you are also informed, Sir, that *the vessels* were secured by ligatures, as they were divided. Although, in your Answer, no notice is taken of this circumstance, I should suppose the object of your inquiry to have been the *blood vessels,* since you have extended the *hydatid existence* to all *encysted tumors.* If this were the object of your inquiry (and, indeed, I see no other connected with the question) may I ask, Sir, for a further explanation as to the hydatid existence of these tumors? I ask it in the way of instruction, because I am much more inclined to doubt the facility of my own comprehension than your perspicuity. However, to me, Sir, there is an evident difficulty in this part of your hypothesis. At one time the distribution of blood-vessels on their surfaces is mentioned (page 59); and at another time, their independency from the surrounding parts (page 74). Are we to consider the containing cysts of such tumors hydatids? or, are we to attribute such a nature only to their contents? That blood-vessels do not shoot into the contents of steatoma, and the rest of encysted tumors, is surely no evidence whatever to support "a separate animal existence," although they may increase and diminish. Such diseased depositions have regulating cysts upon which their increase and diminution depend; and which, in their turn, derive their blood-vessels in common from the connecting medium.† So that viewing *one* as a diseased action,

* Vide Mr. Cline's letter.

† If we refer to less complicated morbid changes, the formation of these cysts would appear wholly and consistently to rest upon a principle of nature;

action, and the *other* as in a state of health, I see no specific difference between a *cell* of adipose membrane, and one of these *cysts*, containing a quantity of morbid fat. In the instance of the adipose cell, we see its contents often *diminished*, and as frequently *increased*; no *blood-vessels* can be traced into such deposition, yet (to use the hydatid phraseology) such "growth" and "decay," were never attributed to any *peculiar property* of *separate life*. If, therefore, we see an adipose cell rapidly increasing its contents, or, at times, as quickly diminishing them, and this without any peculiar properties of *separate life*, attributable either to the *cell* or to its *contents*, I am at a loss to conjecture, why such should not be allowed (precisely the same) to the "cyst," or to the "contents" in steatoma."*

You have drawn, Sir, a comparison between such cysts (when their contents have been discharged, as in meliceris) and natural cavities; such, for example, as the tunica vaginalis

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nature; the shutting out of disease as much as possible from the constitution at large: upon the same rule that the cellular membrane becomes obliterated in its lateral communications, and condensed to serve the purpose of a cyst in common abscess. The different arrangement of the blood-vessels on the surfaces of these cysts, compared to the manner in which vessels are often shortened, and plugged up in the instance of common abscess, cannot surely afford a sufficiency of evidence for the question. A difference in the arrangement of vessels unquestionably is proved; but is it fair to conclude upon this difference, that the whole is an arrangement for a separate and distinct economy? The *one* is a *positive* provision for the while, answering the necessities, and anticipating the accidents of a *temporary* derangement. The *other* is an *unequal*, though systematic effort, to guard against slow and progressive innovation, from the *permanent subversion* of healthy arrangement. The difference in the distribution of the vessels on the cyst therefore, I humbly submit to rest solely and wholly on the necessities of the case, compared to that of common abscess. In the former, no shortening or plugging up of vessels is necessary, because there is no bursting of integuments, nor discharge of contents intended, and therefore no hazard of hæmorrhagy is risked. In the instance of the abscess, Nature sees the evil will redress itself. She says, "all that I have got to do, is to guard against accidents; nothing more is necessary than to wait quietly three or four weeks, and then I shall get rid of the burthen altogether." But in the instance of permanent derangement no such hopes are held out; she sees nothing is to be expected from the part itself; and all that she can do is to make the evil as bearable as possible, by providing against it as well as she can, with what few means she has left.

* There cannot be a doubt (if we admit the hypothesis) that all encysted tumors can only be varieties of the carcinomatous hydatid: since we have daily experience that such occasionally become cancers, therefore such transmutation, together with the marked difference observed in the economy of these tumors in general, and the progress and result of Scirrhi, in common, is left wholly unexplained.

vaginalis testis. By the granulating action of the one, and its quick disposition to heal, you contrast the slow and difficult sloughing of the other; and on this difference you rest a proof of the animalcular existence of these cysts. But here, Sir, the fact only goes to establish a *distinction* between *two* productions; and not by any means a *separate animal economy:* the *distinction* between the living powers of *original* structure, and those of *diseased* formations. No one would ever look for healthy action in a diseased cyst; or, if he did, it is certainly no very great stretch of natural consequences to conclude, that he would be most undoubtedly deceived.* The tedious sloughing of such an exposed cyst can prove nothing for the question. We see the same thing in the slow exfoliations of cartilage: yet, we readily attribute this to a *certain low modification of life*, which such structures possess; and all this without once hinting at the bare possibility of a "separate existence." Alluding, therefore, to the similar proportions of life in the cartilage and the cyst, I cannot conceive, how *that* should establish a *peculiarity* in *disease*, which is freely admitted to be *common* in *health*.†

I beg

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* No proof of a separate animal economy whatever, is afforded by the comparison of the two cysts. The *active* powers given to a *healthy* or *natural* tunic, attach just as much peculiarity to it, as the *want* of such powers can possibly give to a *diseased* one. It is the *presence* of such powers that constitutes *health*; it is their absence which forms *disease*. So that, in fact, the difference in steatoma to a natural cyst, is that of disease to health; and *not*, what is precisely peculiar to the *separate economy* of the part. If this is not so, and diseased actions, on the contrary, are evidence of a *distinct animal economy*; then, most assuredly, the *same nature* must be allowed in the most unequivocal sense to the tunica vaginalis testis (or any other originally formed cyst), when, from accident, it no longer retains that equilibrium of regenerative powers which constitutes health, and which had afforded, by comparison, that *distinction*, upon which the *separate economy* of encysted tumors (according to the hydatid hypothesis) depends.

† If upon so slender a foundation as that of a *single instance* of similitude between a part and an hydatid, we were immediately to attach to the former, a *distinct* and *peculiar economy*; instead of attending to the *necessities* that are absolutely required to make up a *separate existence*, where would *such* a *mode* of reasoning lead us? We should never have an end of it. In natural as well as moral philosophy, the *first step* is every thing. If animalcular notions got, upon such easy terms, into a mind possessing a strong bias, and creative fancy, how very soon the world might be favoured with "A New System." How very soon he would get from diseased formations to healthy structures; and there, with the aids of *analogy* and *modification*, how very soon the whole would be reduced into *separate* and *peculiar* economies. Full scope in the setting out of the hypothesis is granted,

I beg to trespass a short time longer on your patience, by calling your attention, Sir, to some of the known facts connected with cancer; and the mode by which they are explained in the hydatid hypothesis. There is only one way (if one follows such a mode of reasoning, and which you have adopted) to explain the formation of several schirri in the same breast, at, or about, the same period of time, viz. A disposition to the same production, or growth, in several points at once. Upon any other principle of hydatid growth, this circumstance, in cancer, would be inexplicable; for in the hydatis carcinomatosa, we can have *no bursting of a capsule,* from whence the young hydatids might escape *in a fluid,* as in lymphatica; nor can we admit the other mode of hydatid growth, by *any exterior connection* between these small schirrous bodies and the original or parent schirrus, because the interstices are always filled up by the natural structure of the breast, and therefore with each other they can never be supposed to have borne any relation whatever. So that an hydatid reasoner must content himself in all such instances, by the *bare possibility* of the thing, in contradiction to *probabilities.* But even admitting this point to be explained by your hypothesis, experience presents an insurmountable objection to the hydatid existence of cancer. In the extirpation of schirri in many instances, where these little bodies are present, and have been cut into, they have been found still to present a *natural glandular* appearance, sufficiently so, at any rate, to convince the most firm carcinomatous hypothesist, that such could not be bodies of his class. Now, although such bodies are evidently glands in a certain gradation of change from their original conformations, yet it is not less evident from experience, that they

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granted, with regard to contents, figuration, and attachments of bodies; so that he would not confine himself, but suit and vary his terms according to the necessities he met. Thus, proceeding on the pure principle of analysis, he would first parcel out the body into hydatis hepatica, hydatis intestinosa, hydatis musculosa, &c. &c. &c. It would be impossible to stop him; the latitude allowed him in the first instance, extends his analogies, and fortifies his conclusions. Having at length given, with some industry and ingenuity, every part its generic name, his synthetical operations would then commence, and the whole would be re-combined under one general economy; and this, with more facility than might, perhaps, be first imagined, aided, as he would be, by the animalcular proof of the ovum, and the analogous growth and clustering of many animalculæ on the same stem. Thus the last touch would be given to this new and stupendous system—The Hydatid Existence of Man!!!

they would progressively have run into a cancerous state; or, in other words, become carcinomatous. So that the only way one can possibly reconcile circumstances to your hypothesis, in this instance, is to conclude (contrary to all experience) that hydatid birth is the *common,* or *necessary* result of glandular alteration.*

Let us now, Sir, compare the circumstances which oftentimes attend the cancerous wart, or excrescence, and the explanation afforded by your hypothesis.—According to your doctrine (if I understand you right), such a wart must have been, from its origin, a carcinomatous hydatid, or a bunch or congeries of the same. When cancer occurs from such an accident as cutting a small excrescence in shaving, it is generally (I believe I may say always) at an advanced age, and the wart till that time has remained, perhaps, twenty or even fifty years, perfectly in a quiescent state. Comparing this quiescent state with hydatid growth, a difficulty arises, but which, in some measure, is accounted for. You tell us, that the torpid state of many schirri is similar to the state of an egg before incubation. This certainly may be admitted in some instances; though the analogy is by no means correct in all; because some schirri will grow rapidly to a *certain size,* and then become *quiescent.* In short, they present so many varieties in their progress, and are evidently so much under the influence of the constitution, participating in many of its sympathies, affected by the slightest causes, irritated at one time, and soothed at another; frequently suffering a partial absorption, and as often wholly taken up; that every circumstance in their natures stands in open contradiction to an animalcular economy.†

But

* Such doctrine would seem most admirably calculated to support the eternity of animal existence; or, in other words, that there would appear to be a provision in the economy of nature, equivalently to make up for the destruction of one existence by the immediate generation of another.

† Schirri, that have been in four years progressive growth, and have taken on a more evident action the last twelve months, have not afforded, after extirpation, the flesh-like appearance, or "fungus," which one would naturally look for, according to the hydatid hypothesis. For we are told, that this "fungus" is invariably produced (from whence *the progress* of cancer) either to separate a dead hydatid from the living; or, when in an advanced state, to select and parcel them out into distinct groups or families, for the purpose of better preservation. But how many schirri are there of long standing, (whose active growth of late obliged their removal)

But to return from this digression to the cancerous excrescence, we will suppose that the quiescent state of a wart, (viewing it as an animalcular existence) say for forty years, and which from some injury has afterwards become cancerous, is explained. But how can we get over the difficulty, according to the hydatid hypothesis, that a cancerous wart shall have been *several times* injured, either by a scratch, bruise, or a cut in shaving, before the fatal accident, which caused it to run into this state; and yet, from *none of* these, did a *similar consequence* follow. For by the hypothesis we are pursuing, (when cancer has followed the shaving off the top of a wart) we must suppose such an injury, either had become a stimulus of growth to the torpid hydatids, or had so injured one or two of them as to occasion their death, from whence sprang up the peculiar "fungus," appropriated for the parting off the *dead* from the *living*. Now, why this should not have happened (since experience shews, that warts have been previously cut with impunity, before they have become cancerous) in the *first* or *second* instance, as well as the *third*, I cannot comprehend, if I am to be a believer in the carcinomatous faith. No one, I believe, will doubt, but what vessels are directly supplied from the surrounding parts, through the substance of the wart. How is this compatible with the existence of an animal independency?

A natural medium is certainly the only way to view disease; however confined its limits may be, it is undoubtedly the only path we have: all the rest is conjectural; and when once we give the rein to fancy, one conjecture is as good as another.

Reflecting thus upon your hypothesis, Sir, one cannot but feel surprised at the extraordinary provision adopted by nature for the preservation of the hydatis carcinomatosa. Here he cannot but see, that, insead of the hypothesis *following*

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moval) totally without this fungus. In some, where the absorbents have been actively excited, I have known a line of separation running throughout their whole extent, and a third of an inch in depth. Now, one cannot but feel, if the history of this fungus were correct, that after such a devastation in the very heart of their colony, but what some of the hydatids must have been sufferers, and that some of this fungus would consequently be supplied; this, however, has not taken place. For, after removal, nothing could be seen but a white irregular substance (of cartilaginous compactness throughout) intimately connected with the natural structure of the breast, and having a line of separation running unequally through its surface.

following nature, she is made to *follow it.* In pages 44—5—6, and 69, we find her anxiously at work to cherish, prolong, and multiply their existence. To cherish what? To cherish disease! You tell Doctor Baillie, that *his cartilage* in schirrus, before ulceration, is your "primary fungus," generated for the *especial purpose* of fostering the hydatid nurslings; and that when, by ulceration or accident, they become exposed to danger, or to death, a "softer fungus" is *then* produced, which following their growth, carefully cloaks, and selects them into families with all the care of a shepherd, or the anxious solicitude of a mother!

This extraordinary provision for disease, Sir, *alone* would hold me from your faith. I can never be brought to think so ill of Nature. Since the world will have it so, she may be sometimes *whimsical*; her best exertions often fail, and even become diseases; but that she is so base at heart, so palpably false and treacherous to her ends, as your hyypothesis has made her out, I can never admit. All her actions, whatever may be their result, have in the first instance a good intention. When she throws a bag over a stone in the bladder, is it to preserve the stone, or to defend the bladder? Is lamina after lamina of lymph thrown up in aneurism, to prevent the bleeding of the artery, or merely with the intent, at last, from its accumulated bulk, to destroy the surrounding parts? In short, Sir, viewing morbid alterations in general, we find such a uniform attempt *to stop* the progress of disease, instead of *prolonging* it, that such alone is sufficient to exclude the idea of the hydatid existence in cancer; because such an existence can only be supported by imagining a train of operations, disconsonant to general experience, and the known laws of the animal economy.

Even, Sir, your own ability (and which, permit me to say, and not in the way of compliment, is no small proof against your hypothesis) cannot lead you farther with the disease than the breast. After having led one so long through the fertile fields of fancy, your imagination, Sir, is obliged at length to pause. You say, you are at a loss to conjecture for the different appearances of cancer in other parts. Permit me to remark, that, if you had kept in view the original structures of the cancerous breast, and the cancerous lip, their difference in health might have suggested to you a reason for their varieties in disease.

The points of objection to your hypothesis, I submit to be these:

1st.

1st. The solitary fact of the contraction of the cells found in cancerous fat, is not alone sufficient to establish their animalcular existence.

2d. The intimate connection of these cells with the surrounding structure, proves their supply to be in *common* with such parts; and, therefore, such an existence is in direct opposition to an animal independency.

3d. The description and contents of these cells differ so little from, indeed, are so very similar to common adipose, that, so far from their being a *subsequent* production; on the contrary, they would appear to be nothing more than *original* structure, under the change of a morbid progress.

4th. That such an existence is not necessary to the disease, inasmuch as such cellular appearances are not *uniformly* present in cancerous alterations.

5th. The many varieties of the disease; its undefined and irregular progress; the various and dissimilar dispositions of schirri; their frequent participation in the common events of the constitution; the avowed and marked effect of general as well as local means on their nature and structure, appear wholly to exclude the idea of an animalcular economy in cancer.

6th. The admission of such an existence in cancer, renders it necessary to imagine a chain of operations wholly disconsonant to the views of the animal economy, viz. A creation for the sole purpose of preserving and prolonging disease; at the same time that we must entirely exclude, by such an admission, all idea of the known morbid progress, which naturally results from the permanent derangement of parts.

7th. and lastly, Because (although every objection be placed aside, and every position admitted) the hypothesis, after all, is itself imperfect: inasmuch as it is unequal to provide reasons for many of the phenomena attendant on cancer; and when viewed in a collective sense, it is even found inadequate to account for the *common* or *local* varieties of the disease.

These, Sir, are my reasons for rejecting the hydatid existence of cancer, and for the further support of my attempt to explain the origin and progress of this disease, which I have ventured to submit to the public, and which, I know, has been honoured by your perusal.

I am, &c.

March, 1806.

SAMUEL YOUNG.

To

To the Editors of the Medical and Physical Journal.

GENTLEMEN,

THOUGH there is scarcely any disease that has more engaged the attention of physicians than rabid hydrophobia, yet we have still to lament, that on no subject whatever have medical learning, skill and ingenuity, been exerted with effects more disproportionate; for it is undeniable that notwithstanding all the remedies hitherto proposed, either as prophylactic or curative, the malady still continues to occur, and to prove as intractable as at any former period.*

Many cases have, it is true, at different times, been published, in which cures are said to have been performed; but if these statements be examined with attention, we shall, for the most part, see reason to suppose, that the authors of them had mistaken some other disease for hydrophobia.

Of this kind, according to our apprehension, is the recent account given by Professor Rossi, in his reports of the efficacy of galvanism in two cases of supposed rabies.† The same remark, it appears to us, is equally applicable to an account of a case of hydrophobia, communicated by Dr. Burton, of Bent, in Virginia, to Dr. Rush of Philadelphia, and published in the 78th Number of the Medical and Physical Journal. It is therein asserted, that the disease, in this instance, was effectually combated by copious bleeding and mercury.

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* When treating, in the 73d Number of the Medical and Physical Journal, of the external means employed in the way of prevention against hydrophobia, we took occasion to reprobate, as an error fraught with the most dangerous consequences, the conduct of those practitioners who recommend, in the most unqualified manner, particular modes of practice to the exclusion of the only certain preventive of this dreadful malady, the excision or destruction of the bitten part.

The arguments contained in that paper apply, it must be admitted, with equal force to the different internal remedies, hitherto so frequently extolled as specifics in preventing this formidable disorder, since their inefficacy is fully proved by the appearance of the disease after they have been administered with the greatest care. Hence too much pains cannot be taken to correct the existing prejudices in their favour, and to inculcate, on every occasion, the danger that may ensue from allowing such an ill-grounded confidence to supersede the timely and effectual removal of the injured part.

† Medical and Physical Journal, No. 67.

 After

After the strictures by Dr. Bardsley on Professor Rossi's Reports, (Med. and Phys. Journal, No. 69) we deem it wholly unnecessary to enter into any farther investigation on that subject, and at present intend to confine ourselves to the consideration of the case related by Dr. Burton, on which, we trust, we may be permitted to offer such observations as a candid and impartial review of the subject shall appear to us to demand.

In the case to which we here allude, we must first observe, that no proof whatever is adduced of the absolute madness of the animal, when the patient sustained the injury; it is merely mentioned, that he had been bitten by a dog, which his friends suspected to be mad; but on what this conjecture was founded, we are left in complete ignorance. It is a fact verified by repeated observation, that scarcely one animal out of forty, supposed to be rabid, is really so; which affords an unequivocal proof how little reliance can be placed on the testimonv of patients or their friends on such occasions. Unless, therefore, proper measures be pursued, so as to ascertain, beyond contradiction, the madness of the dog, doubts must always arise, as in the present case, how far the symptoms that may supervene, are imputable to the bite of a rabid animal.

It has also in general been too rashly inferred, that when persons are injured by animals actually rabid, hydrophobia must necessarily occur; but this by no means follows, for it is an indisputable fact, that out of twenty persons so injured, nineteen on an average escape without becoming affected by the disease.

We are not told in this case, that any mode of prevention whatever was employed, so that the patient was probably left to indulge his apprehensions of the consequences of the accident; which appears, from the whole tenor of the account, to have made a deep impression on his mind.

The bite, we are informed, was inflicted on one of the lower extremities, but nothing is said respecting the state of the bitten part; whether it looked red, assumed an inflamed appearance, or underwent any change previous to the attack, or during the continuance of the complaint. We are indeed told, that the patient, after his convalescence, informed Dr. Burton, in answer to some inquiries made by him to this purpose, that he had felt a slight pain attended with an itching and uneasiness in his groin, for several days previous to his illness; but when it is considered,

sidered that he made no complaint of this kind, either before the attack or during the intervals of the paroxysms, as they are termed, nor was ever observed putting his hand to the bitten part, which is not unusual with patients labouring under this dreadful malady, very little importance, it should seem, can be attached to this account; more particularly, when the evident prepossession of the patient, as to the nature of his disease, is taken into consideration.

The length of time which intervened between the infliction of the bite and the accession of the complaint, in the present instance, is stated to have been twenty-four days, which is a shorter interval than usual; for, in well marked cases of hydrophobia, the disease seldom makes its appearance sooner than five or six weeks after the accident. As, however, much may depend on the susceptibility of the system at the time the injury is sustained, the virulence of the poison, and various other circumstances, considerable latitude must, it is obvious, be allowed as to the period of the attack. But when, along with the absence of most, if not all, of the diagnostic symptoms of true rabies, any unusual deviation occurs in this respect, as was the case with Doctor Burton's patient, it affords additional ground to suspect, that the symptoms may proceed from the operation of a cause wholly unconnected with the application of hydrophobic virus.

Several days previous to the appearance of the disease Dr. Burton, we are told, was informed that the patient had been dull and solitary, and that his friends, a few minutes before the attack, had found him two hundred yards from the house, apparently in a deep study. This fact, while it tends to prove that the mind of the patient was from the first in a very disturbed state, evidently militates against the supposition of rabid hydrophobia; for it is a leading symptom of this disorder, that the patient cannot bear exposure even to the air of a common room, without experiencing great uneasiness.*

The symptoms enumerated by Dr. Burton, when he first visited the patient, were, a dull pain of the head, watery

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* This extreme aversion to the contact or application of cold air or moisture did not escape the notice of some of the more early writers on this subject. Among other symptoms, Cælius Aurelianus mentions it as one of the signs of approaching hydrophobia: His words are, "Insueta etiam querela aeris, tanquam austrini, quamvis serena fuerit quies; item difficilis toleratio, atque tædium, & recusatio imbrium."

eyes, dull aspect, stricture, and heaviness at the breast, and a high fever; all which are common to a variety of disorders, and display nothing characteristic of rabid hydrophobia. We are indeed immediately afterwards told, that the patient rejected every thing aqueous; and in a subsequent letter to Dr. Rush, who had requested more precise information respecting several particulars, we are given to understand, that he refused to swallow liquids, and that the sight of water threw him into a convulsive agitation. But, admitting this statement to be correct, it does not thence follow that the malady, under which the patient laboured, was rabid hydrophobia; for it is well known to every intelligent practitioner, that such symptoms are frequently concomitants of other diseases. Innumerable instances of this kind might be adduced; but as the fact is too generally known to render that necessary, we shall only mention a few, which appear to us more particularly remarkable.

Boerhaave relates a case, in which the difficulty of drinking, and consequent dread of water, accompanied an acute fever, occasioned by excessive heat and fatigue, superadded to the intemperate use of ardent spirits.* A very singular case is recorded in the Edinburgh Medical Essays, vol. i. in which hydrophobia appears to have depended on an inflammatory state of the stomach, without the most distant suspicion of the patient having been injured by any rabid animal. Whether this might be in reality a case of gastritis, appears somewhat problematical, for few of the diagnostic symptoms of that affection were present; but be this as it may, the patient frequently ejected saliva from his mouth, and experienced all the horror and anguish at the sight of water, which are so conspicuous in persons labouring under genuine hydrophobia. A case of hydrophobia supervening to a phrenitic attack is related by Dr. Raymond in the Appendix to the 5th vol. of the Medical Observations and Inquiries. We ourselves once witnessed this occurrence, during a phrenitic delirium, in a patient who rejected water and other liquids with a kind of horror, as often as they were presented to him. The late fever at Leghorn, according to Dr. Palloni, frequently manifested itself by the appearance of hydrophobic and other extraordinary symptoms. While these and other facts serve to explain many of the various and contradictory

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* Van Swieten Comment. Tom. iii.

tradictory reports respecting the effects of different remedies in rabies, they evince the necessity of a clear and accurate discrimination between it and other apparently similar diseases; since there seems but too much reason to suppose, that various affections of an essentially different nature have frequently been confounded with it, merely because the patient had felt an aversion to water at some period of the disorder.*

Whoever attentively examines all the particulars of the case related by Dr. Burton, must, we think, be convinced that he has fallen into this error, and mistaken an affection of a very different nature for genuine hydrophobia. From the very commencement of the disease, the delirium of the patient appears to have been attended with furor; he raved almost incessantly, and bit at every thing indiscriminately within his reach; he was bled; blisters were applied; and in every respect he was treated on the antiphlogistic plan. But although, by these means, the violence of the complaint was somewhat abated, it recurred with increased aggravation on the morning of the third day from the attack, when we are told, he was found biting the bed-clothes; that his countenance was maniacal, his pulse synocha, with stricture of the breast, difficult deglutition, laborious breathing, and a discharge of saliva. All these alarming symptoms were, however, at last, effectually subdued by copious and repeated bleeding: mercurial frictions were also resorted to; but the complaint had evidently yielded before they were employed.

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* What has proved the most frequent source of this error, we have every reason to suppose, is the application of the term hydrophobia to designate the disease itself: a term, which taken in its literal and etymological sense, is merely descriptive of a single symptom, which, so far from being confined to true rabies, is not an unusual concomitant of various and very opposite affections. With a view to obviate this source of error, various terms have been proposed by different writers on rabies as a substitute for that of hydrophobia, but all of them appear to be subject to more or less objection. It would, indeed, be difficult to find a single term sufficiently comprehensive fully to express that state of the system which is induced by the action of the rabid virus. Hygrophobia (Ὑγροφοβία), which the Greeks themselves employed, is perhaps less exceptionable than most others, in as much as it is more extensive in its import, and expresses not only an antipathy to water, but that preter-natural aversion to all fluids, whether in an aerial or more palpable form, which is so characteristic of true rabies; the latter term, it must be confessed, is somewhat less euphonous than the former, but if it be preferable in other respects, that is a consideration of very inferior importance.

During the short space of four days, the patient lost 138 ounces of blood.

From the history of this case it appears sufficiently obvious, that violent inflammatory action and acute pyrexia prevailed throughout the whole progress, and constituted the principal part of the disease: circumstances which are never observable in well defined cases of hydrophobia.—On a careful review of the whole concourse of the symptoms, as detailed by Dr. Burton, we are of opinion, that they must appear to every impartial observer far more characteristic of a phrenitic attack than of true rabies; for in phrenitis, the raving is almost incessant, with few intervals of reason; whereas in rabies, the paroxysms are short and sudden, and the patient is seldom bereft of all consciousness and perception of what is passing around him. In phrenitis, moreover, the delirious furor early manifests itself, and the patients are generally outrageous; but in rabies, on the contrary, timidity is a prevailing feature, and the patient seldom attempts to injure any of the byestanders, or becomes outrageous, till towards the very close of the malady. The only symptoms in which the disease under consideration bore the slightest resemblance to rabies, were, the patients refusal to swallow liquids, and the convulsive agitation he is said to have experienced at the sight of water. Now these symptoms, as already shown, are no uncommon occurrence in phrenitic, as well as in maniacal affections. In phrenitis, besides, it is well known, that the mind frequently recurs to objects which had before made a deep impression on it. That this was the case in the present instance, is clearly evinced by the patient continually warning people to keep at a distance from him, for he had been bitten by a mad dog. We are fully aware that the desiderium aut cupiditas mordendi is given as a part of the character of this disease by several Nosologists, and that many writers consider it as a proper and inseparable sign of rabies; but whatever stress may be attached to this circumstance by some authors, it can only be considered as a symptom of delirium which is common to several disorders, and therefore furnishes no proof of the patient having been affected with canine madness.

With regard to the discharge of saliva in this instance, it seems altogether to have arisen from impeded deglutition, and the convulsive action of the muscles of the throat and fauces, and is well known to be a frequent occurrence in epileptic as well as in maniacal and phrenitic affections;

affections; to the last of which the disease in question appears to have had a more striking resemblance, not only in the mode of its attack, but in its subsequent symptoms, than to the peculiar malady resulting from the bite of a rabid animal. It was not marked by any of the leading symptoms of hydrophobia rabiosa; such as an aversion to noise and cold air, to the sound or contact of water and other liquids, to any violent contractions of the lower jaw, any strictures about the throat, with occasional apprehensions of immediate suffocation or strangulation, any violent efforts to disengage from the mouth a thick and viscid saliva, any change of voice, incessant restlessness, and a marked aversion to a recumbent posture. Lastly, it not only differed from this dreadful disorder by the absence of all these symptoms, but in its termination, which was so unlike that of true rabies, hitherto deemed incurable, that it yielded in less than a week to copious and repeated bleedings, and the employment of other remedies:—means which, in the hands of other practitioners, have uniformly proved inefficient. On the whole, so far as we can form any judgment from a comprehensive view of the present case, there seems, we think, every reason to conclude that, whatever might be the disease under which the patient laboured, it was not genuine hydrophobia.

I am, &c.

R. HALL.

Clement's Inn,
March 28, 1806.

REMARKS *on the* INTERNAL USE *of* CANTHARIDES *in* GLEET *and* LEUCORRHŒA, *illustrated by* CASES. *By* JOHN ROBERTON, *Surgeon, Edinburgh; and Member of the Royal Medical Society.*

MUCH dispute, controversy and error would be prevented in medicine, as well as in the other departments of science, if ideas were expressed with more perspicuity of language, and the subjects of inquiry described with more precision than is usually done. The truth of this remark is well exemplified in what authors have said concerning the medical properties of Cantharides, which have been with equal ardour condemned as the most pernicious, or praised as the most salutary of substances, and, perhaps, with equal judgment also; since the one approved

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without understanding in what circumstance, or why it was useful; and the other rejected, without inquiring why it had been unsuccessful. This, like every other most powerful agent, when injudiciously or rashly employed, must have very pernicious effects, but when rationally and deliberately, its effects are not less conspicuously salutary.

In the consideration of every inflammatory disease, it is of the utmost importance to distinguish accurately between inflammation properly so called, and its consequences, wherefore we venture thus to distinguish gonorrhœa and gleet, or blenorrhagia.

Gonorrhœa, a contagious disease, distinguished by an active inflammation affecting the urethra, producing a discharge of puriform contagious matter from that passage.

In gleet, there is an atony of the urethra, the effects of active inflammation or other debilitating causes, accompanied with a glary or ichorous discharge, which is not contagious.

It seemed unnecessary to express these definitions in such a manner as to suit both sexes, since the mind can easily make allowance for the difference of structure, and perceive that the facts signified are equally applicable to both.

These two conditions, in principle and in practice, are completely analogous to the states of active and passive inflammation, as distinguished by the most correct and systematic writers; nor is there any rule of medical practice more firmly established, than that in the first, strong stimulants are as hurtful as they are beneficial in the second.

Accordingly, we need not be surprised that one person using the most active substance of cantharides in the gonorrhœa, as above defined, should meet with chagrin and disappointment, and condemn it in gonorrhœa; while another, who used it in gleet, which was also named gonorrhœa, should, as we do, from undoubted experience of its superior efficacy, recommend it to the notice of practitioners as a most valuable addition to the resources of the healing art.

We might have suspected from the relation of the contradictory effects, which this substance is described to have produced, what was the source of the error; but if this suspicion be corroborated by the facts contained in the writings of those who have prescribed it, the gonorrhœa has been confounded both in name and

and in practice with gleet. If, where cantharides have been unsuccessful in removing discharges from the urethra, these have been gonorrhœas, but where successful, gleets; in fine, if positive experiment prove, that by inducing gonorrhœa, it cures gleet; will not this point be established, that gonorrhœa being confounded with gleet, is the cause of the opposite sentiments of writers with regard to the effects of cantharides in certain discharges from the urethra?

In the same manner we may, I presume, reconcile the difference of opinion with regard to the use of the corrosive muriate of mercury in gonorrhœa.

That the word gonorrhœa was employed to signify the discharge from the urethra in both the conditions above pointed out, the slightest attention to authors will amply satisfy any one; indeed, the word etymologically denotes a flow of semen, and has been applied indiscriminately to all discharges from the urethra; it is only in the advancement of science that the necessity of words precisely defined becomes urgent.

Dr. Greenfield * relates many cases of gonorrhœa, which he and others cured by means of cantharides; but if we examine the histories of the affection which they treated, we shall be convinced that they are those of gleet: and the phrases, violent gonorrhœa, virulent, painful, &c. which other writers employ to designate the affections in which the cantharides were unsuccessful or hurtful, show that these were discharges accompanied with active inflammation, or real gonorrhœas.

Lastly, I have not only the testimony of authors, but have found by indubitable experiment, that cantharides induce active inflammation with puriform discharge before gleet is remedied; how then could authors but fail in their attempts to remove active inflammation using a most inflammatory substance?

On examination of the facts, we firmly believe that those pernicious effects which have succeeded the use of cantharides, and which excited such horror and aversion to its exhibition in disease, have, as in the instance of gleet and gonorrhœa, principally originated in the want of discrimination between inflammatory action and its consequences.

Previously

* On the internal use of cantharides.

Previously to my employing cantharides in gleet, I had not consulted authors on the subject, nor am I ashamed to confess it, since the life-time of any individual would scarcely be sufficient simply to read over, far less to examine, the merits and doctrines of one twentieth part of the books written on the powers of particular medicines; besides, when we consider how useless the most of them are, a man has not much reason to regret, that he is not endowed with dull perseverance enough to pore over them.

Having since, however, examined the works of authors on this subject, I am happy to find that the experience of some of the most eminent medical men has found cantharides as efficacious a remedy for gleet as I have done; among these I may mention Bartholine, Hoffman, Lister, &c.

When I first employed cantharides in gleet, I only knew that this substance was a powerful stimulant taken internally, and produced painful effects on the urinary organs; and as pain generally indicates a certain degree of inflammation, I thought, that in inveterate gleets, where there is both general debility and exhaustion, with torpor and atony in the parts of generation, this substance might have considerable influence in restoring both the general and local activity; and the experiment, when made, exceeded in success my utmost hopes.

In order to demonstrate the truth of this assertion, I shall relate one case selected out of many; averring, that when exhibited with due caution, the cantharides effect the cure without vomiting, strangury, or any of those alarming symptoms, which we have been taught to dread, as the inevitable consequences of its administration.

S——d, aged 55, a small meagre man, on the 20th of April, 1804, mentioned to me that he laboured under an uncommonly severe gleet, at the same time saying, that he had no hopes of cure, for he had tried all the common prescriptions without advantage; but as the effects of the disease were now very severe, he was willing to submit to any thing likely to afford him relief; as, to use his own phrase, "life was become a burthen to him." I inquired minutely into his case, and found, that he had been affected with a discharge of this nature from the urethra, for about twenty years, which he attributed to the effects of a bad habit, originally commenced at school, and since aggravated by repeated claps.

Such was his situation, that besides a continual discharge from the yard, emission succeeded the most trifling erection,

erection, and straining at stool had the same effect, which was followed by a great languor and depression of spirits.

Though married, connubial enjoyment was beyond his faculties; but this was not his only misfortune, head-ache, loss of appetite, lumbago, incontinence of urine; in fine, general emaciation and debility threatened the speedy termination of his life.

All the most common means had been already employed to remove his complaint; accordingly, to make him undergo a repetition of the same, was neither consonant to my feelings, nor suitable to the state of my patient, I therefore prescribed as follows:

℞. Tinct. Cantharid. ʒiß. Sp. Lavend. comp. ℈j. Aq. font. ℥vij. M. Cap. coch. mane et vesp.

This quantity having no effect, the dose was increased on the 23d to ʒij.; this likewise had no effect, and on the 26th was augmented to ʒiiß. which, though taken in the course of two days, an increased dose was still necessary. On the 28th, ℥ß. was prescribed with the above quantity of water, yet ineffectual; and this being all taken, the quantity was on the 30th increased to ℥j. of the tincture, which was taken in four days, and at this ratio he continued to the 8th of May. Between the 8th and 10th, he took half an ounce per diem; but inflammatory symptoms of the urinary organs appearing, for the first time, the dose was diminished to ʒij. in the day, and so on to the 17th; and whenever the signs of inflammation commenced, the matter of the gleet assumed the appearance, in some measure, of laudable pus, accompanied with chordée and severe erections, but yet no degree of stranguary; but what was somewhat remarkable, the erections were not now followed by those emissions, languor, sadness, and sudden impotence, which had been previously so certain consequences of that state.

The discharge from the urethra having now the form of the matter of a most inflammatory gonorrhœa, it was deemed unnecessary to persist in the use of the remedy, and unequivocal inflammation being indicated, it seemed prudent on the 18th to employ an injection, for the purpose of mitigating the symptoms; accordingly, a scruple of the sulphate of copper dissolved in six ounces of water, was ordered to be used three or four times a day, and, contrary to my expectation, the cure seemed to be complete in the course of four days from this date.

He left Edinburgh soon afterwards, and repaired to London, where his wife was, and in a short time he had

an increase of his family. He remained perfectly well in January, 1806.

Having for several years found the cantharides a very useful remedy in gleet, I became imperceptibly acquainted with the phenomena which succeeded its admission into the system; I had uniformly observed that it rendered the mind more cheerful, strengthened the circulation, promoted the animal functions, appetites, and evacuations, and greatly mitigated that sense of general debility, as well as of pain and feebleness in the loins and joints, of which, those who have long laboured under this affection so much complain: these are the first effects; but in a space of time, longer or shorter, according to the state of the patient, the discharge begins to be inspissated, pain attacks the urethra, erections are frequent, and even accompanied with some degree of chordée; but these erections are not as formerly, of short duration, succeeded by languor, debility, depression of spirits, and spontaneous evacuation of a semen like fluid, but precisely resemble the erections in gonorrhœa, and the matter discharged is puriform, and often, after erection, mixed with semen, particularly during the night.

While my mind was impressed with these facts, some obstinate and very inveterate cases of leucorrhœa came under my care; and reflecting that the constitutional symptoms of leucorrhœa are precisely similar to those of gleet, the matter of the same appearance and consistence; and as the gleety discharge evidently depends on a debilitated state of the mucous membrane of the urethra, so does the leucorrhœal on that of the mucous membrane of the vagina; in short, that all the circumstances were completely similar; it appeared to me very probable, that cantharides might also remove leucorrhœa by invigorating the general system, and producing a change in the action of the mucous membrane concerned, as happened in gleet, and the event confirmed my opinion.

Mrs. L——n, aged 25, a little woman, mother of five children; the first lived only a few hours after birth; the three following were still born; and the last lived only about half an hour. Her general appearance indicated languor and debility; her eyes peculiarly dull and heavy; pulse feeble. On the 9th of October, 1805, I learned that she had laboured under fluor albus for five years and a half, and the attack commenced about two months before the birth of her first child.

She has, during that time, employed those remedies which

which are usually prescribed in this disease; but notwithstanding, her complaint had gradually increased in violence, and was at length intolerable. The opinion of the medical men she had consulted, had induced her to believe that a complete cure was scarcely to be expected. Her pain of back was excruciating; her appetite impaired; and so far from being able to walk, she could not even stand or sit, without the utmost uneasiness. The discharge per vaginam too, was so copious and incessant, that, though she used cloths, &c. she was always afraid of being in company, lest marks on the floor should discover her condition; in addition to all which, she was frequently attacked by paroxysms of hysteria.

The discharge was of a glairy appearance and consistence. During the whole time of the affection, the menstruation has been regular and natural; and what is worthy of remark, the fluor albus evinced itself much worse during pregnancy than at other times, showing, as I would venture to allege, that the particular state of the mucous membrane of the vagina alone has no small share in promoting the disease.

The patient says, the flow was not in uniform quantity, but that an unusally great discharge was always preceded and announced by an excruciating pain in the region of the kidnies, on which occasion she always had the distinct sensation of something flowing, as it were, downwards from the loins, and the cloths applied to the parts were found wet with the discharge.

Is there not reason to conjecture that the mucous membrane of the urethra is equally affected with that of the vagina?

As in other cases I had been accustomed to use the tincture of cantharides with great success, I prescribed as follows:

℞. Tinct. Cantharid. ʒiiß. Aq. font ℥vj. M. One table spoonful to be taken thrice a day.

October 12. Cantharides mixture repeated with ʒjv. and aq. font. ℥vj. and again repeated with the same quantity of cantharides on the 16th.

17th. She felt slight pain in the uterine region, extending down the vagina, particularly on making any exertion. The discharge was now somewhat thicker and whitish.

19th. Cantharides mixture repeated; in the evening the pain became very troublesome, even when sitting, and the discharge was much thicker and whiter. I now prescribed as an injection:

℞. Sulph.

℞. Sulph. Cupri. ʒß. Aq. font. ℥vi. M. To be thrown into the vagina thrice a day.

20th. Discharge and pain almost gone; when I desired her to take two tea spoons full of the cantharides mixture every three hours, till the pain should return: but before she had taken a sufficient quantity to produce this effect, the discharge resumed almost its former violence; cantharides increased to the quantity prescribed on the 16th.

21st. Discharge considerably diminished, attended with slight pain.

22d. No pain, although she has taken within the last twenty-four hours ʒiv. of the tinct. cantharid. The discharge now less, by more than two-thirds, than it was previously to the exhibition of the cantharides. I now ordered the injection to be omitted, and the following increased dose of the tinct. cantharides to be used.

℞. Tinct. Cantharid. ℥j. Aq. font. ℥vj. M. To be used as the former mixture.

23d. After taking one dose of the last mixture, she was for the first time sensible of pain in voiding urine. This was the period for the return of the menses, which accordingly appeared, but she was at the same time seized with a fever, and species of cynanche tonsilaris, that had raged epidemically in Edinburgh for some weeks. Tinct. cantharid. and injection now intermitted, as he fever, &c. continued to the first of November.

Nov. 2d. Fever gone; throat getting better after one of the tumors had suppurated and burst.

Menstruation still continuing, and of an alarming quantity, prescribed this day,

℞. Acidi Nitrici ʒij. Aq. font. ℔ij. M. One wine glassful thrice a day, till the flooding ceases.

7th. Flooding ceased; the discharge of the fluor albus considerably abated.

9th. Discharge much aggravated. General stimulants were now prescribed, because the system was much reduced by the fever, &c.

12th. Discharge much diminished,

13th. Discharge entirely gone; the general health much improved.

17th. Discharge returned in a slight degree, and of the same appearance and consistence as before the use of the cantharides.

21st. After earnest persuasion, on my part, she was prevailed on to recommence the tinct. cantharid. in small doses.

22d. Great sickness during the night; and inflammatory symptoms, with a puriform color in the discharge, came on before morning.

℞. Camphor, G. Mimosæ Niloticæ, aa. ℈j. Alcohol. q. s. ft. Massa in pilul. xv. dividend.

One of these pills was taken an hour after each dose of the cantharides mixture, which was to be taken thrice a day.

23d. Cantharides continued; very great pain this day; but as it has never become so severe as on the 21st, she has used none of the camphor pills.

24th. Discharge entirely gone. Cantharid. continued in smaller doses, but sufficient to maintain the suppurative action.

25th. Pain of head.

℞. Supertart. potass. ℥j. solve in jusc. bov. ℔j. To be taken presently; cantharid. continued; no return of the discharge.

26th. ℞. Tinct. Cantharid. ℈v. Aq. font. ℥vj. M. To be taken as formerly.

On the evening of this day, after having fatigued herself by walking, and having also neglected to take the cantharides since morning, the pain in the uterine region abated, and the discharge returned in a slight degree.

27th. Cantharides to be taken during this day, till the pain returns; pain and considerable difficulty in making water came on this night at bed time, with nearly a complete cessation of the discharge.

28th. Cantharides continued, and one of the camphor pills to be taken; discharge slight.

28th. Cantharides continued; discharge not entirely gone, but nearly so.

30th. Discharge entirely gone; cantharides continued, and the quantity last prescribed, repeated; considerable uneasiness, or rather pain in making water. Partial sickness this day and yesterday, with head-ache; supertart. potass. as above, which relieved these symptoms.

December 1st. Very early this morning there was an almost imperceptible discharge; cantharides and injection continued.

2d. Uneasiness extending along the vagina and uterine region, still kept up by the use of the cantharides; injected two or three times since yesterday, but no return of the discharge,

3d. The pain in the region of the kidnies, which heretofore invariably indicated the returning discharge, was felt this morning, but not followed by the discharge, or even the

the smallest appearance of matter, per vaginam; in other respects as yesterday.

The menstruation not having returned at the regular period, there is reason to suspect the intervention of pregnancy; I have therefore ordered the dose of the cantharides to be diminished, but the injection into the vagina to be continued.

4th. Had a violent attack of hysteria this morning, occasioned by mental agitation. Such an attack never happened before, during the complaint, without its being succeeded by an uncommonly copious discharge, of which there is no appearance. Medicines to be continued as last prescribed.

5th. No discharge; uneasiness of the parts gradually abating.

6th. A repetition of the hysteric paroxysm from a similar cause.

7th. Early this morning the discharge reappeared, of a puriform nature, and continued to augment all this day; pain from the cantharides almost gone. Cantharides to be taken in increased doses, and the injection to be assiduously repeated.

8th. Discharge gone; pain of the parts returned; remedies as last directed, but in diminished doses.

10th. No discharge; pain gone; general health much better than she thinks it ever was since she laboured under this affection; is not oppressed with that despondency from which she felt very disagreeably in her former pregnancies.

14th. No cantharides taken since the 10th; pain and discharge gone; complains of sickness in the morning; loss of appetite; formerly when pregnant, she had none of the usually concomitant symptoms of that state, except the cessation of the menses, and the increase of size, at which time too, the discharge per vaginam was signally augmented.

20th. Previously to this date she had used the injection very irregularly. During the afternoon of yesterday, she had been walking, sailing, and dancing a good deal; suddenly awoke this morning with violent palpitation of heart, and agitation of mind, from a dream; slight return of the discharge; injection to be used more regularly; cantharides to be repeated in small doses.

22d. Vomits the cantharides immediately on taking it, and disgusted with it. Her common food is loathsome to her, and excites severe sickness and vomiting, but no pain in

in the stomach or chest. The same occurs on getting up in the morning; same discharge, though scarcely perceptible. Cantharides discontinued on account of pregnancy; injection used once a day.

January 1st, 1806. Discharge has continued as on the 22d ult. She danced and fatigued herself a good deal this evening, but this was succeeded by no augmentation of the discharge, as on the former occasion, when she had been sailing and dancing. Could the cold sea-air produce any difference in the result? for on the first of January she fatigued herself much more than on the former occasion.

5th. Says the discharge is still perceptible. I asked her what it was, compared with what it had been? She replied emphatically, "It is not the ten thousandth part of what it formerly was; and I feel myself as well in my general health as ever I was in my life, except that my bones and joints are still sore from the exertion of the first day of this year."

The above case was selected, because it indicated the greatest variety of circumstances; but there are others, which shall afterwards be detailed, of equal duration and obstinacy, which have been completely cured without relapse.

I may mention that cantharides seem formerly to have been used successfully in leucorrhœa, though the nature of the affection which this medicine removed, does not seem to have been understood by those who employed it. Dr. Greenfield, for example, had adopted a notion, that almost all such discharges as those of gleet and leucorrhœa depended on ulceration of the bladder, and that cantharides were an infallible remedy for such ulcers, just as in our own times some promulgate, that almost all affections of the organs of generation, nay, even of the head, stomach, bowels, &c. depend on strictures of the urethra, for which bougies and caustics are the only specifics.

The following case, communicated to Dr. Greenfield by Charles Bernard, Surgeon to Queen Anne, was evidently one of leucorrhœa mistaken for ulceration of the bladder.

"A certain lady of quality, who for a long time had been afflicted with an ulcer in her bladder, and had applied herself to many of the College of Physicians in London, but without any manner of relief; she mightily implored, from time to time, my assistance, which I as often denied, because the distemper was both difficult of cure, and chronick withal, and the patient daily voided an in-

credible quantity of mere fœtid pus; at last I was prevailed upon to undertake, or rather attempt the cure, and the more so, that I might have the opportunity of trying the method so mightily recommended by Dr. Greenfield, viz. the internal use of cantharides in curing ulcers in the bladder. I began the trial by two grains of cantharides, to be taken twice a day, from the 26th of March to the 2d of April, but by the use of these a dysuria ensued, wherefore we omitted them till 24th April, and prescribed balsamics in their room; but what was very specially observable was, that the quantity of purulent matter (which she used daily to evacuate) was wonderfully diminished.

"From the 24th till the 26th of April, was repeated two grains of cantharides twice a day as before.

"From the 26th of April till the 1st of May, we advanced to three grains of cantharides, intermingling balsamics by intervals; meanwhile a strangury continued to be very troublesome, but the purulent matter lessened daily, and at length wholly disappeared."

Having collected a much greater number of facts and observations than could be admitted into a periodical publication, or than my other affairs would allow me immediately to prepare for the press, I intend still further to prosecute the present inquiry, and during the intervals of professional employment, to prepare for publication the result of my future experience on this subject.

Edinburgh, 1806.

To the Editors of the Medical and Physical Journal.

Gentlemen,

Permit me, through the medium of the Medical and Physical Journal, to make some observations on a paper in your last Number, denominated, "Extracts from the Proceedings of the Institution to promote Cow-pox Inoculation in the town of Nottingham," but more particularly on the latter part of that paper, *extracted from a letter addressed to the Secretary of the Royal Jennerian Society*; without the most remote idea of appearing before the tribunal of the public.

Although

Although a long extract has been made from the Proceedings of this Institution, yet, what may be considered as a very material part, the Regulations, are omitted; certainly the machinery, by which the movements are directed: they are extremely simple, and as such more valuable: I shall claim your permission to insert them.

"At a meeting of the Directors of the Institution for Vaccine Inoculation, held for the first time on the 31st of October, 1805, it was agreed, that for the better attainment of the purposes of the Institution, and the better regulation of their proceedings, the following general Rules be observed.

"1. That the Directors shall meet twice at least in every month, for the purpose of receiving Reports of the state of Vaccination, of inspecting the records, and adopting such measures as shall appear to them to be the best calculated to promote the success of that practice.

"2. That the Directors of this Institution do not propose to take upon themselves any share in the executive part of this plan, except that the medical members of it shall at all times be ready, when called upon, to give their advice, either individually or in consultation, on any question of doubt or difficulty that may arise in the practice.

"3. That a Physician be appointed to the Vaccine Institution, and that it shall be his duty to be be ready to give his advice, when called upon by the surgeon; when any doubt arises respecting the propriety of inoculating any individual, or in any case of irregular symptoms occurring during the process, to see each patient that is vaccinated under the direction of the Institution, at least, once during the process; and, that more especially in its advanced stage, that he may be prepared to give his opinion how far it has proved satisfactory or otherwise; and to request, whenever he may judge it necessary, the assistance of any one or more of the medical members of the Institution, to decide any question of doubt or difficulty that may arise.

"4. That a Surgeon be appointed to this Institution, whose duty it shall be to visit all the poorer families of the three parishes in the town, in regular rotation, beginning at some one part of it; to encourage and persuade them to accept the advantages offered by the Institution, and to vaccinate as many as are willing and are deemed fit subjects for this practice; to visit each patient that has been vaccinated, at three distinct periods after vaccination, and

 make

make as many written Reports on the case, adding such observations on each case as shall be thought necessary by the physician and himself.

"In case of any irregularity occurring in the progress of the symptoms, to call upon the physician for his assistance, and to keep a regular register of each case in a tabular form, including the names, age, and dwelling of each patient, and three distinct Reports of the progress and observations as above stated."

These Resolutions of the Directors require little comment; under their benign influence 903 persons have been inoculated.

The Public Register of the Institution has twelve columns, a blank page for remarks, and a case-book for those of more importance; such trouble may be considered superfluous, yet that labour is not to be regretted which puts us in possession of such accuracy, as to defy calumny when or where it may arise.

The poor are vaccinated at their own habitations:—without such an arrangement this charitable Institution would be almost nugatory. The surgeons here had in the most laudable manner devoted two hours once or twice a week to gratuitous inoculation; this was found very inadequate to their benevolent views; few can give up that time which is so necessary to an attendance on the surgeon; this neglect prevented that regular inspection, which is indispensable to ascertain the progress of the disease; and from conviction of the inefficacy of that plan, an Institution was established.

Of the Address to the Inhabitants of Nottingham, a part only is inserted; I have no doubt it was observed, that a similar statement had been made to the public; a repetition was certainly unnecessary in a publication of such extensive circulation. But its efficacy here, among the lower class, to whom above 4,000 copies have been distributed, is beyond our expectations; it appears to carry conviction to their minds, and few, very few, hesitate to receive the blessing *Jenner* has so liberally bestowed on mankind.

I shall proceed to make some observations on the Extracts from my Letter, and on the Notes of Dr. Walker; but first let me say, that I shall be flattered by any communications from your ingenious Correspondents.

In page 255, line 5, "The progress of the disease," I have said, "was soon observed to be much retarded, either by very cold weather, or by much exposure to cold, particularly

particularly in debilitated children, who form the majority of the poor of this place."

Dr. Walker's Note does not offer any explanation, but an additional and well known cause of this retarded state.

Page 255, line 26, "Each patient is visited four times; on the third visit, which is generally on the tenth day, a purgative of jalap, or jalap and calomel in combination, is prescribed; we were induced to adopt this plan in compliance with popular prejudice;" and Dr. W. should have added, "but as the children of the poor are generally of very gross habits, we have often found the fever and erythema moderated, as well as the child improved in its general health;" and I may now say, that farther experience has proved to us its great utility.—Dr. W's commentary on this passage is only referable to the practice of the Royal Jennerian Society.

Page 255, line 33, "But when from the test and one or more inoculations no effect is produced, they are considered unsusceptible of cow-pox."—Dr. Walker's note is extremely just; when we are obliged to reinoculate, two or three punctures are made in each arm, inserting fresh fluid ichor; even if we do not then succeed, they are only marked as at that time insusceptible of cow-pox; and if they have not been formerly much exposed to small-pox, so as to render it doubtful if they have not had that disease, or what the poor call the small-pox fever, (which is fever during an exposure to the disease without any perceptible eruption), they are reinoculated at a future period; for this necessary perseverance we are indebted to Dr. Walker.*

The distinction between vesicle and pustule, page 256, line 1, is a subject of much importance, and leads to many practical conclusions: In the usual progress of the disease it is a *vesicle* before the appearance of the erythema and induration;† the fluid will then become *puriform*, which gradually concreting forms a *scab* of a dark mahogany colour.

It has been recommended when the *vesicle* is ruptured to stop the the farther discharge by applying to it a drop of the aq. lythar. acet.‡ this, when used undiluted, proves

* Vide Medical and Physical Journal, vol. 12, p. 45.

† Willan, on Cutaneous Diseases, Def. 10th and 12th.

‡ Address of the Royal Jennerian Society, p. 49. Bryce on Cow-pox, p. 226.

a powerful escharotic; but will answer the purpose when mixed with water. A troublesome ulcer has in some cases followed the rupture of a *pustule*, which we are advised to bathe with the aq. lytharg. acet. comp.* I am inclined to suspect that this application, and the constant exposure of the naked arm, may, in some constitutions, by suddenly stopping the discharge, lay the foundation for an absorption, which produces great induration and enlargement of the axillary glands, which sometimes suppurates, but generally heals favourably.

The only method we have found to prevent this absorption is by the application of the unguent. spermat. ceti to the ruptured pustule, and treating it in every respect as a simple ulcer; but when absorption to any extent takes place, in conjunction with this application a brisk cathartic generally proves effectual.

The Note at the bottom of page 256, demands most serious consideration. "In the number which it continually falls to my lot to inoculate," says Dr. W. "it is generally extremely easy to determine by the eye when the effect is complete; but, when the pock has been ruptured, and even almost obliterated, if I can feel, about the tenth day, a degree of hardness and tumefaction about the inoculated part, I find in people of every colour that the protection is complete; and without such inflammation (induration?) I suspect that the effect is never complete."—This is an opinion entirely new to me, presenting a perfect diagnosis between the true and what has been called the spurious cow-pox; and if I understand the Note, the appearances before or after the tenth day are of little importance, so that on the tenth day a degree of hardness and tumefaction is felt about the inoculated part: I confess I may interpret Dr. W's language too literally.

Mr. Drew relates two cases,† in which induration, with the other usual marks of the true disease, regularly appeared; from which Dr. Walker, with others of great experience in vaccination, pronounced the *protection complete;* yet both these cases afterwards had the small-pox.

Not taking this opinion to the extent mentioned, if it be only applicable when a *pustule* is ruptured, the *vesicle* having proceeded regularly, we are under much obligation

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* Address of the Royal Jennerian Society, p. 50. Bryce on Cowpox, p. 231.

† Medical and Physical Journal, vol. 14, p. 537.

tion to Dr. W.; for nothing in vaccination has given us so much trouble as this state of uncertainty arising from ulceration, We shall speedily bring it to the only certain test,—Experiment.

Page 256, line 7, "We have not found any inconvenience to attend inoculation during teething, hooping cough, &c. provided that the fever of these affections be not violent."—The hooping cough has lately been very virulent, and I believe in every case where the disease was present at inoculation, the vesicle assumed the peculiar flat surface as before related, the pustule and scab presented the same shape and appearance, but in every other respect was as usual. In two or three cases where the hooping cough appeared some days after inoculation, the disease proceeded without any unusual occurrence. I have not seen this peculiar vesicle in any case but when associated with acute disease.

In page 26, speaking of the peculiar scab of a greyish-brown colour, I have remarked, that it is generally observed in persons of a scorbutic habit;—since that Report I have had an opportunity of seeing it on persons in perfect health;—a pretty good idea may be conceived of it by a reference to Willan's first plate, fig. 3.

To account for this variety in the scab, Dr. W. refers me to an explanation in the Med. and Phys. Journal,* where it is supposed to be altered in its nature by some then existing eruption.—I am sorry I cannot give my support to this explanation; persons affected with what are called herpetic eruptions are recommended to the Infirmary, and are not inoculated until I have succeeded in removing these diseases.

The amber coloured scab is of two kinds,—the first follows the rupture of a vesicle, and cannot be depended on;—the second arises form a ruptured pustule, and differs from the regular scab only in colour.

In the Report of the 9th of January, in the case of Charles Judge, page 257, line 20, *vesicle* by an error of the press is used for *pustule;* a term, as there applied, contradicting the distinction made in the former part of this paper.

In the case of Elizabeth Hall, is not the small-pox to be considered as the constitutional disease, and the cow-pox as only local? I am, &c.

Nottingham, JAMES CLARKE,
March 12, 1806. Physician to the Vaccine Institution.

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* Medical and Physical Journal, vol. 14, p. 537.

To the Editors of the Medical and Physical Journal.

Gentlemen,

A Composition which I communicated to the Royal College of Physicians, when they revised their Dispensatory, about twenty years ago, and which they did me the honour to adopt, is now come into general use; and considered by many practitioners, as one of the best active cathartics and anthelminthics: I therefore apprehend, that a few remarks on this composition may not be unacceptable to your readers.

I have now used it above thirty years; and experienced the superior advantage of it in many hundreds of cases, on account of its activity, and the great ease with which it is given to children. This latter circumstance is of no small moment, when we consider how delicate their stomachs are, even in a state of health; and how much their sufferings are sometimes aggravated by a nauseating drug.

In the specimen published by the College, and submitted by them to the perusal of medical practitioners, the old formula, called Pulvis Basilicus, was inserted; and proposed to be restored to the Pharmacopœia, where it formerly held a place. Having been favoured with the perusal of the specimen, I drew up a few remarks on this composition, and presented them to Sir George Baker, then President of the College, who communicated them to the Committee appointed to revise their Pharmacopœia. I had afterwards the satisfaction to learn from him, that my reasoning on the subject met with the approbation of the Committee; and particularly of that ornament of the College, and of the medical profession, the late Dr. Heberden.

I stated my opinion, that scammony is not a drug which requires a corrector, as had been generally thought; and that if any such corrector were necessary, the mode in which it is applied in the pul. basilicus would still be objectionable. The formula consists of equal parts of scammony, calomel, crystals of tartar, and washed calx of antimony. If an acid is requisite, in order to correct scammony, the absorbent is detrimental; and if an absorbent is requisite, the acid is detrimental. It appears to me, that the calx of antimony and crystals of tartar, neutralising each other, while they add to the bulk, render the composition less efficacious, and much more unpleasant.

It

It is usual to give a powder of this kind in honey or currant jelly; and there can be no other objection to such a mode of administering it, but the difficulty. I have generally directed it to be mixed with a small quantity of water in a spoon, or to be made into a draught; and have seldom been disappointed in the operation. Two drachms of water are sufficient for this purpose; and the calomel in this composition is so well combined with the scammony, by means of the sugar, that very little of it will be lost, provided the phial be properly shaken.

In the basilic powder, the calomel used to subside, even in the bottle where it was kept in a dry form; an objection to which the present formula is less liable; and on that account it has met with the approbation of some of the first characters of the present age.

When I first altered the basilic powder into this form, it was for the sake of giving it with more ease to children who were under preparation for the small-pox; and also after that disease. The necessity of such a medicine, if it ever existed, is now in a great measure superseded by vaccination. But it has other uses; and it has been found practicable to administer this medicine in cases, where a composition of calomel and jalap could not be given, on account of its unpleasant flavour.

These two articles constitute the basis of many nostrums for worms, by which the public are so much deluded. The medicine under consideration is at least equally efficacious, and less disgusting; I have, however, frequently known injections of aloes prove successful in the cure of ascarides, when medicines administered by the mouth had failed; and am inclined to believe, that it is a mode of cure which ought in general to be preferred, because it does not carry off the nourishment contained in the stomach and intestines, nor do so much injury to the constitution in other respects, as violent cathartics exhibited in the usual form.

Cases of the Small-pox a second Time.

Many false alarms have lately been propagated, in consequence of local small-pox excited in those who had previously undergone vaccination; especially when such local small-pox was attended with fever and other constitutional symptoms. On this account I have already communicated several instances, where the same circumstances occurred in those who had undergone the small-pox; and now add two other cases to the number.

Mrs.

Mrs. Frankham, No 1, Mount Pleasant, Gray's Inn Lane, had the small-pox when young, and is considerably marked with it. Since that time she suckled a child labouring under the disease; in consequence of which, she had several pustules from the contact of matter, which went through the regular course; and were attended with head-ach, fever, and all the other signs of constitutional indisposition, to a considerable degree, for many days.

Mrs. Dean, No. 21, Park Street, Grosvenor Square, was inoculated for the small-pox. Her arm rose; and she had many perfect pustules, attended with the usual constitutional affection. Since that period, she had a number of pustules on her chin, and other parts, by contact of matter from a child at her breast. These pustules regularly maturated, and turned on the 9th day, leaving very large and deep pits behind. She had also the symptomatic fever, and other signs of constitutional indisposition to so great a degree, that the late Mr. Whale, of Oxford-street, who attended her, could scarcely believe she had had the disorder before. This, however, there could be no reason to doubt; had it been otherwise, the pustules, on the last occasion, would not have been merely local.

A case of the small-pox after variolous inoculation, similar to that in the family of the Earl of Westmeath, occurred in a daughter of Mr. Rowan Hamilton, who was inoculated in Ireland, in the year 1787, by Dr. Daniel, a regular physician; and supposed to have the disorder in the most complete and perfect manner. In the month of November, 1806, she caught the small-pox from her brother; and apparently had it a second time.—This case was communicated by Mr. Rowan Hamilton to Mr. Murray, Secretary to the Royal Jenerian Society.

Burserius says, vol. 3, "They also entertain an erroneous opinion, who think, that after once having the genuine small-pox, the disposition of the body to receive that disorder is destroyed; for it appears from undoubted facts, and the investigations of medical men of unquestionable authority, that not a few persons, after experiencing the complaint in the natural way, or by inoculation, have afterwards been affected a second, and even a third time. Hence it is evident, that this disposition is not always destroyed, after once having the disease; or at least, that it may sometimes be excited again."

The following cases were communicated to Dr. Walker by Dr. Huggan, in a letter dated Wallingford, Berks, Jan.

Jan. 2, 1806. They were related to him by an intelligent woman, who was an eye witness to them.

——— Woodley, a native of Harwell, in that county, where his mother still lives, had the small-pox in London, when he was about 25 years old. His face and hands were covered with large broad pustules full of matter. Fifteen years afterwards he was again attacked with the small-pox, when settled at Reading; of which he died. His brother, who went from Harwell to see him, brought back the infection; which rendered a general inoculation necessary at that place.

The woman from whom Dr. Huggan received this information, resided in London about thirty years ago, and was inoculated at the Small-pox Hospital, together with another woman, of the name of Mary Pink, a native of Lewes. They both had the disorder in the regular way; with pustules on different parts, which maturated and turned in the usual manner. About a fortnight after they were dismissed from the hospital, Mary Pink was again attacked with fever, followed by an eruption of the small-pox. She was blind several days; but at length recovered. Another woman living in the next parish to Harwell, was a witness to this circumstance; and recollects it perfectly well. Hence, Dr. Huggan justly observes, it is evident, that the inoculation of the small-pox is no more infallible than that of the cow-pock.

A daughter of the woman who communicated the foregoing cases, slept with her sister who had the small-pox, during the whole course of the disorder; in consequence of which she had two pustules on her knee. She was therefore considered secure from future infection; but three years afterwards she again had the small-pox of the confluent kind; and narrowly escaped with her life.—When she had the local pustules, she laboured under a sore throat, attended with fever; which probably rendered her for a time insusceptible of the constitutional affection of the small-pox; and it is no wonder, that the same thing should now and then happen in the cow-pock.

The following is a case, which I lately received from Mr. Merriman, in a letter dated Jan. 20, 1806; and was communicated to him by Mrs. Dawson, of Shepherd's Market. It occurred to an elderly woman, of Hanslope, in Buckinghamshire, to whom Mrs. Dawson formerly went to school. She perfectly recollects having had the small-pox when 17 years old; which was so severe, as not only to produce pits, but even seams in the face.—Three years

years ago she again caught the distemper, when it was epidemic in the town; and had it in so violent a manner, that her life was despaired of. Mr. Dawson's brother, a surgeon at Hanslope, attended her in this second attack; and Mrs. Dawson herself saw her during the eruption.

Another instance of this kind was communicated by Capt. Mowat of Eastbourne; by whose desire the following letter was since transmitted to me by Mr. Verral, surgeon, of Seaford.

"Sir,

"I write to you at the request of Capt. Mowat, to relate the case of Mr. John Wilson, Jun. of Bletchington, whom I attended some time since with the natural small-pox; which he affirms to be the second time of his having that disease. Mr. Pierpoint of Linfield, who inoculated him, is dead; but I have written to Mr. Ward, a practitioner in the same place, who formerly lived with Mr. Linfield as an assistant. This day I had an answer from him, in which he says, that from Mr. Pierpoint's books it appears, Mr. Wilson was inoculated, with two other persons, on the 17th of April, 1783; and left the house on the 7th of May. Mr. Ward recollects nothing of the case; but makes no doubt that the usual appearances took place; and that Mr. Pierpoint was satisfied of his security.

"Mr. Wilson's own account is much more decisive. He says there were above thirty patients with him in the pest-house; he was ill, had a great number of pustules in all parts of the body, and did not recover in less than three or four weeks. He has since been frequently amongst the small-pox; and seen the eruption of that disorder; which he declares to be similar to that which he had in consequence of inoculation.

"In April, 1804, some soldiers of the regiment in Bletchington barracks had the small-pox; and Mr. Wilson, having business there, was much exposed to the infection. On Saturday the 28th of that month, after a day or two of previous indisposition, an eruption began to appear, which he thought resembled the variolous eruption he had seen; but his mother, apprehensive that if he raised an alarm of that kind, it might deter customers from their shop, prevailed on him to confine himself to his room; and no medical man was consulted till the Wednesday following, when they sent for me. When I saw him, I had no hesitation in pronouncing that he had the small-pox; and its progress was from that time so regular, as not to leave a probability of my being in an error. "The

"The nature of the disorder was, however, afterwards proved in a remarkable manner. His mother, with whom he lived, had never had the small.pox. As I was at that time vaccinating many patients in the village, I advised her to submit to that operation; but this she refused to do, and wished me to inoculate her from her son. This, in my turn, I stedfastly refused; as I prided myself that my hands had never been guilty of communicating that disease. We parted, both obstinately adhering to our resolution; but when I visited her son in the evening, I had made up my mind to yield to her prejudices, rather than leave her any longer exposed to the danger with which she was threatened. I assumed, however, for a time, the appearance of being unaltered in my determination, and had the satisfaction of converting her to my opinion.

"She was accordingly vaccinated, and her arm rose and inflamed in the usual manner. On the eighth day an eruption appeared, which, if I had never seen a similar one before, would have alarmed me much. It was a variolous eruption; the consequence of exposure to that infection previous to and during the progress of the cow-pock. Two or three pustules on the face and bosom maturated, the rest began to die away a few hours after their appearance; and by the time the areola was fully formed round the vaccine vesicle, they had entirely disappeared. She had a little fever; perhaps rather more than usually attends the simple cases of cow-pock; but she was not ill enough to require the least confinement.

"Mr. Wilson had a considerable degree of fever, and the eruption was copious; but the disease certainly appeared to be lighter, than is usual with the natural small-pox. This may perhaps be considered as a corroborating evidence of the identity of the first disease; which we may suppose to have weakened, but not entirely to have eradicated the variolous disposition.

"If you wish to make any farther inquiries, I beg you will consider me to be one, who thinks nothing a trouble, that may tend to promote the cause of vaccination, of philanthropy, and of trnth.

I am, &c.

C. VERRAL."

To these cases of the small-pox a second time, I have one more to add. It occurred in a young woman who was inoculated at Chelmsford, when seven years of age. She was ill one day, and had about twenty pustules. Many

others

others were inoculated at the same time; and no doubt was entertained that they all had the small-pox. Five years after, a woman under inoculation came to the house where she was; and she caught the disease a second time, in a much more severe manner than the first; having a confluent eruption, which has left considerable marks behind. I am not permitted to mention the name of this person; but a reference will be given to her, upon application to Mrs. George, No. 23, Great Castle Street, Oxford Market.

These cases appear to me to be well authenticated; but not more so than the two cases of a similar kind, published in your Journal for March, by Mr. Simpson of Skipton; to whom I beg leave to return, by the same medium, my grateful acknowledgements for his valuable communication.

Mr. Moore, in his Reply to the Antivaccinists, informs us, that he is acquainted with a lady, who had the small-pox from inoculation in her infancy; and who has since had a variolous affection six times, from suckling her children successively, when under inoculation. The pustules filled; and were not distinguishable from the ordinary small-pox. "In one sense therefore," says he, "some persons appear to be susceptible of the small-pox, as often as the contagion is powerfully applied. But this secondary species is commonly a slight indisposition: the fever is usually inconsiderable, the pustules few, small, and soon disappear.

"In the multitude of constitutions, as some are endowed with a singular degree of morbid irritability, these are of course violently influenced by a trifling stimulus. But except in very extraordinary instances, the secondary variolous affection is much milder than the primary: consequently it appears, that an essential change has taken place in the habits even of those who catch it; which occasions a feebler action of the poison. In confirmation of this it should be recollected, that these secondary attacks almost always proceed from the natural infection; which is apt to excite a far more malignant disease than inoculation.

"Many gentlemen of high distinction in medicine, and particularly many strenuous friends of vaccination, infer from such cases as have been taken notice of, that some individuals have the small-pox twice. I know not whether they will be satisfied with the distinction I make, between the primary and the secondary species; for which there

appears

appears to be a real foundation. I feel inclined not entirely to abandon the principle, that the human body is insusceptible of the true virulent small-pox twice, which has been established by millions of examples; especially as the occasional exceptions admit of an explanation conformable to the rule Upon this subject, however, I am not very tenacious. There is but a shade of difference between my opinion and theirs; and both of us totally reject the notion of the antivaccinists; who positively deny the recurrence of the small-pox in any shape. These gentlemen appear to be enamoured with this loathsome and destructive malady. They will not suffer any reproach, however just, to be thrown out against it; but flatter it, as they would a deformed heiress."

That there is a clear distinction between the primary attack of the small-pox, and that which usually occurs, must be readily admitted; but the cases which I have here stated, and many others on record, are sufficient to prove that a second attack of the small-pox, in its virulent form, is not so very extraordinary as it is generally supposed to be. In confirmation of this, I shall here subjoin a concise account of some of the most remarkable instances that appear to be authentic.

The first was published in the fourth volume of the Memoirs of the Medical Society of London, in a letter from the late Mr. Withers, surgeon, of Newbury. From his account it appears, that Mr. Richard Langford, of West Shefford, in Berkshire, had the small-pox when a month old, at the same time with three others of the family, one of whom died of it. His face bore the most evident marks of the malignity of the distemper, being so remarkably pitted and seamed as to attract the notice of all who saw him, so that no one could entertain a doubt of his having had it in the most violent degree. Supposing himself to be particularly secure from any future infection of the disorder, he used to frequent the houses of the poor people in his parish, in order to attend them, and accommodate them with such necessaries as their cases required.

In the month of May, 1775, Mr. Withers was sent for to see him; and was informed, that about a fortnight before he had been indisposed for two days, from over-heating himself; but recovered, and continued well till the day before Mr. Withers saw him. "He was then seized with chills, pain of his back and head, &c. with a considerable degree of fever." On the fourth and fifth days his fever

fever was somewhat abated, and all the other symptoms mitigated. On the sixth he was still better; and Mr. Withers was informed, that early on the morning of the preceding day they had perceived a rash, which they forgot to mention, and which had escaped Mr. Withers's notice, the chamber being very dark. This eruption was not confined to the face, but extended to his arms, breast, and body. It so much resembled the small-pox, that Mr. Withers told the family he should not have hesitated to pronounce it to be this disorder, if it were not notorious that Mr. Langford had already had it.

The next day the eruption was universal. The throat, of which he had complained the day before, was now become more troublesome; and every other symptom so far favoured the idea of the disease being the small-pox, as to induce Mr. Withers to give the most decided opinion of its being so, and to desire that he might have no intercourse with any of his friends who had not had the disorder. This opinion was ridiculed, and little attention was paid to the precaution; nevertheless, the progress of the pustules, the swelling of the face and head, and the smell peculiar to the disease, as well as every other circumstance, still more and more confirmed Mr. Withers in the opinion he had formed.

On the eighth day two physicians, Dr. Collet and Dr. Hulbert, were consulted, both of whom declared the complaint to be the small-pox; but neither Mr. Langford nor his friends could be reconciled to this idea. The consequence of which was, that the advice of the medical practitioners was not followed, and the patient died on the twenty-first day.

Five of the family being seized with the small-pox, one of whom died, the people in the neighbourhood were convinced that the disease was the small-pox. It was thought so extraordinary a case, that the Rector has recorded it in the parish register.

Mrs. Cook, of Kemp's Court, Berwick Street, was well acquainted with Mr. Langford, and with all the particulars of his case. She declares, that previously to the second attack, he was much pitted, scarred, and seamed with the small-pox. The same account has been given to me by others. Mrs. Cook also informs me, that another woman, besides his sister, caught the small-pox of him; and died of that distemper.

This case, if there were no other on record, is, I humbly presume, abundantly sufficient to prove, that the human body is not " insusceptible of the true virulent small-

pox

pox twice." But other instances of a similar kind are not wanting. One, which I have already noticed in a former communication, is related by Dr. Buchan; who informs us, that he knew a nurse, who had the small-pox before, so infected by lying in the same bed with a child who had a bad kind of the small-pox, that she had not only a great number of pustules which broke out all over her body, but also a malignant fever, which terminated in a number of boils and imposthumes, from which she narrowly escaped with her life.

• Another remarkable case of this kind was published in the Medical Journal for May, 1801, by Mr. Purton of Alcester; who observes, that his father, Mr. Bloxham, a man of sound judgement, who had been extablished in a very extensive business forty years, and attended the patient with him through the whole course of the disease, was equally convinced of its real occurrence a second time. Mr. Purton had before heard of a case of the kind; which was well authenticated, and sufficient to stagger the firmest sceptic.

In the present instance, the patient received the infection of the small-pox, each time, in the casual way. In the first attack, her mother caught the disorder from her; and narrowly escaped with her life. This happened twenty-two years before the second attack; and she had the disease very violently, as the marks which remained sufficiently attested. On the 11th of March, 1801, the whole village of Exhall was inoculated for the small-pox; and this young woman was appointed one of their nurses. On the 3d of April, she was attacked with severe rigors, pain in the head and back, sickness, and the other usual symptoms of an approaching fever. The two following days she was extremely ill: the fever increased to an alarming height, attended with delirium through the greater part of the night, previous to the eruption, which appeared early on the 6th of April. It continued to increase for several days. Her throat became sore, with a sense of fulness, from the number of pustules covering the fauces; which continued filling till they were completely maturated. The pustules on the face began to turn on the ninth day of the eruption; and those on the other parts of the body a few days after.

If any case can be stronger than those which are specified, it is that which occurred at Arundel, which I communicated to the public a few months ago, through the medium of this Journal. In that case, the patient was

twice covered with the small-pox, at the distance of more than sixty years; and twice *apparently* died of this cruel disease.

The case of Mr. Miles, surgeon, of Dursley in Gloucestershire, published by Dr. Jenner in his Inquiry, is also worthy of attention. This gentleman, who had the small-pox by inoculation, and inoculated not less than two thousand persons, at different times, had reason, according to the notion which then prevailed, to think himself secure from the future infection of that disease. But having inserted a little variolous matter into the back of his hand, in order to show how he performed the operation, a pustule arose; and on the eighth day he was seized with all the symptoms of the eruptive fever, in a much more violent degree than when he was inoculated eighteen years before. As a proof that the first inoculation was effectual, he had a great number of pustules. On the tenth day of the second inoculation, he felt a very unpleasant sensation of stiffness and heat on each side of his face; which terminated in three or four pustules, attended with inflammation. The pustules did not maturate.

The two following cases were communicated to me by Dr. Jenner; and published in my Answer to Mr. Goldson. Mr. Fewster of Thornbury, the celebrated inoculator, had the small-pox in his youth, and was exposed to the infection with impunity for forty years; yet, happening to wound his finger with the point of a lancet, charged with variolous matter, the puncture inflamed and suppurated; and he had a considerable number of pustules on his forehead.

Mr. Scott, another surgeon in the same town, had the small-pox by inoculation; which proved to be of the confluent kind. Nevertheless, he caught it again twenty years after, from a patient whom he was attending; and had it with some degree of severity.

The following case is related by Mr. Hickes, in his Observations on a late publication of Dr. Pearson. It took place five or six years ago, at a farm-house at Arlingham, in the county of Gloucester. A boy had been inoculated with variolous matter, and had been covered with eruptions. About a year after, some of the family having the small-pox, he caught the disease again, and had a very heavy burden.

Mr. Dunning published a case, in which there is great reason to believe the small-pox occurred a second time. Elizabeth Everitt nursed people labouring under the small-

pox

pox thirty years. This woman supposed she had had the disorder when a child; and there is great reason to believe that this opinion was well founded. Yet she caught it at length, and died. Dr. Geach had related a similar case to Mr. Dunning; which happened to a nurse at the Royal Hospital at Plymouth, under his own observation.

The following case was this day communicated to me by Mrs. Mead, No. 23, Addle Hill, Doctor's Commons; who about twenty years ago, lived in the family of Mr. G. Smith, then residing in Clifford Street. A black woman, who was a native of India, and lived in the same family, being attacked with a violent disorder, told Mrs. Mead, she thought she was going to have the small-pox; which she had already had twice; and of which she bore evident marks. She added, that if it proved to be this disease, she should certainly die; that in India they sometimes had it three times, but in that case they always died. Her words were prophetic; for she had the small-pox, which proved fatal.

The following communication, which I lately received, also tends to corroborate the opinion I now entertain, that the small-pox a second time is no uncommon occurrence.

"*Princes Street, Cavendish Square*,

"Sir,

I have sent you four cases, containing a plain narrative of facts. I cheerfully submit them to you, from a conviction that they will be employed, as far as they are capable, for the advancement of truth and science.

I am, &c.

William Royston."

"*Cases of casual Small-pox succeeding to the inoculated Disease, after an Interval of Nineteen Years.*

Sarah and Mary Taylor were inoculated for the small-pox; Sarah when two years, and Mary when four days old. Sarah was inoculated by a Mr. Richardson, who was engaged in practice with, or was a pupil of Sutton. She had but few pustules, and those faded early, but she was pronounced safe. Mary was inoculated by Mr. Robert Muriel, a respectable country practitioner, and the virus which he used was taken from a person who had the disease by inoculation. She went regularly through the distemper, had a considerable crop of pustules, and an indelible mark remains in her arm where the matter was inserted. During a period of nineteen years these young women,

women, perhaps, were not exposed to the small-pox contagion; but at the end of that time, in February 1788, a general inoculation took place in the village where they resided. Sarah was then attacked with the casual disease; the pustules were confluent in her face, but distinct in other parts; she was blind some days. Mary took the disease at the same time; the pustules on her were numerous but distinct, and turned on the second day.

"*Case of Small-pox occurring a second Time.*

Mary Feakins, of Somersham in Huntingdonshire, had the small-pox in the year 1769, the disease being then very general in the neighbourhood. It appeared in her in its severest form, and she suffered extremely. When she was attacked with the complaint she nursed an infant daughter, then six weeks old, who took the small-pox from her mother, had a confluent kind, was much marked, and lost an eye. In the year 1788, when this daughter was nineteen years old, on being exposed to the accumulated contagion of the small-pox, she was again attacked with the disease; and I attended her. This second attack was not of the slight kind which has been observed to occur in nurses. The eruptive fever continued four days; there was considerable inflammation about the fauces; the number of pustules in her face amounted nearly to two hundred, with a proportionate crop over the surface of the body. The pustules on the face arrived at the acme of maturation on the seventh day from their appearance. Swelling in the face closed the eyes; the extremities were tumefied, and some degree of ptyalism took place. The accumulated effluvium to which this girl was exposed occurred from her sleeping in the same room with three younger sisters, who were down with the disease, one with the casual sort, and two by inoculation.

"*Case of Change in Idiosyncracy from an* insusceptible *to a State* susceptible *of Variolous Contagion.*

Mr. T. C. was inoculated for the small-pox with his family; the punctures on his arm assumed a morbid appearance, but he had no febrile symptoms. During the progress of the disease in the family, one or more of those who had pustules on them, constantly slept with Mr. C. From this test he was deemed safe from the impression of variolous contagion, either from the essential change produced by inoculation having silently taken place in him, or

or from some peculiarity of idiosyncracy. Twelve years after this period he became exposed to variolous contagion, from the casual disease appearing in his family. He then took the small-pox, had it in its severest form, and nearly lost his life.

" I now beg leave to subjoin a letter which I some time ago received from Mr. Blair; as a supplement to the account I have already given of the case of Jowles. It relates to the publication of Dr. Rowley, who is now no more. But that is no reason, why the misrepresentations which abound in his work should not be corrected. This is an act of justice to the individuals who are concerned; and to the public."

" *Great Russel Street, Feb.* 11, 1806.

" Dear Sir,

I thank you for the opportunity of perusing Mr. Moore's 'Reply to the Anti-Vaccinists.' I observe, that he notices one of the two cases, respecting which you enquired of me, viz. that of a poor boy called Jowles, " whom Dr. Rowley has so cruelly nick-named the Cow-poxed, Ox-faced Boy, and of whom he has published so hideous a caricature." Mr. Moore very justly denominates it a case of scrofula; for it is nothing else, and there is nothing peculiar in it, unless it be that the boy has tumefied eyelids, which produce a staring aspect, and a redness of the tunica conjunctiva. It is a very singular and far-fetched conceit in the learned Doctor to imagine, " that this boy's face seemed to be in a state of transforming, and assuming the visage of a cow." During the months that I have had the treatment of him at the Bloomsbury Dispensary, his visage has improved, and some of his scrofulous complaints are amended.

" The other case, which you enquire after, is that of a young girl, who had been vaccinated by Dr. Thornton; but unfortunately, after having remained several years in good health, she fell down and broke one of her ribs, which occasioned an abscess in the chest, and for which I am now attending her. I do not know precisely what Dr. Rowley has said or written about this case of Empyema; but I learn from Dr. Thornton, that the public have been here likewise deceived as to the real state of the child, and that her present infirmity has been attributed by Dr. Rowley to the Cow-pock Inoculation!

" Probably you asked for my opinion on these *two cases*, in order to undeceive some persons, who had taken up a false

false idea respecting them. The hints I send, if they be deemed worthy of attention, are very much at your service: for I feel it an incumbent duty to proclaim real facts as they exist, and am not unwilling to pledge my reputation in support of the truth of what I affirm. The events I now relate are no more the effects of Vaccination than the consequence of cutting the navel string at the birth of the two children in question. I am, &c.

W. Blair."

I have many other Observations to offer concerning Dr. Rowley's Work; but these I reserve for a future paper.

I am, &c.

New Street, Hanover Square. JOHN RING.

An Account of a malignant Fever, which appeared in the Garrison of Quebec during the Autumn of 1805, *with some preliminary Observations on the Diseases of the Canadas. By* E. Walsh, M.D.

THE climate of Canada is stated, in every account of the country, as almost proverbially severe; and the extremes of heat and cold are described as meeting with scarce any intervention of moderate temperature. Such accounts are very far from being correct; yet there exists a much greater conformity of climate throughout the whole extent of the country, (now divided into the two provinces of Upper and Lower Canada) than might be supposed, in a range of 10° from north to south, and 30° from east to west.

This identity of climate may be attributed to the gradual ascent of the land towards the west, and to the great evaporation from those immense fresh water seas, which, every winter, are partly frozen, or covered with drift ice, and whose accumulated waters find their passage to the ocean, in forming the broad, deep, and rapid river, St. Laurence. They reckon, however, a month's difference of season between Quebec, in lat. 46° 47′ N. and Niagara, lat. 43° 15′ N. But this difference does not affect the general constitution of the atmosphere. The course of the winds and the seasons are equally variable in both, and the greatest summer's heat in either is about 92° of Fahrenheit; the medium 70°; whilst the mercury falls every

very winter to at least 16° or 18° below zero of Farenheit at Niagara, and to 25° at Quebec. The medium heat of the year is about 55° throughout the whole extent of the Canadas.

The entire country is intersected with rivers and lakes, which have all a communication with the river St. Laurence. On the borders of these widely diffused waters the settlements are formed; the rest is primæval forest. These settlements are extremely numerous on the banks of the St. Lawrence; many of them are more than a century old. There is no population on the face of the earth, which enjoys a greater portion of health and freedom from disease, than the inhabitants of the old settlements. Canada is indisputably the healthiest division of the Western Continent. It was the first that was colonized in North America, and the only colony of Europeans, with perhaps the exception of that recent one of New South Wales, which had not to encounter the ravages of endemic diseases.

The diseases of Canada partake of the nature of the country. They are few, but strongly marked. In Upper Canada, intermittent, remittent, and an anomalous species of the latter, called by the natives *the Lake Fever*, are prevalent. They are indeed, in most situations of the Upper Country, epidemical every autumn.

The American Physicians attempt to demonstrate, that these are gradations of one genus of fever, of which the ague and the yellow fever form the extreme links. This theory is plausible, inasmuch as the three first mentioned must be referred to one exciting cause; viz. Marsh Miasmata.

But these genera of marsh fever in the upper country are becoming, happily, every year, of less frequent occurrence; not only on account of its rapid settlement, but from a remarkable phenomenon of the lakes themselves. These vast reservoirs of fresh water are gradually but perceptibly diminishing, so that the stagnant waters of their marshy borders now find a descent, by which they are spontaneously drained into the basons of the lakes. Thus large tracks of unwholesome fens have been, and continue to be, converted, with scarce any labour, into beautiful meadows, affording luxuriant pasture to numerous herds of cattle.

It would be superfluous to describe the remittent and intermittent fevers of this country: the latter is of the tertian type, but more refractory and irregular than that usually met with in Europe; whilst the bilious remittent,

on the contrary, is generally milder, and yields easier to treatment than that genus of fever in tropical climates.

Some account of the Lake Fever, however, so called from being endemic only in the neighbourhood of the lakes, may not prove unnecessary. The access of this fever is not marked, like the ague and remittent fever, with strong and decisive symptoms; the diagnostics, in the beginning, are in general faint and obscure; it comes on with dull pains in the head, chills seldom increasing to rigor, lassitude, a sensation of soreness all over the body; the cuticle appears rough and dry to the feel, the tongue white but moist; the heat of the body not increased, nor does the pulse differ from that of the natural standard of health. But the most remarkable diagnostics of the lake fever are, its irregular illusive remissions, and its lengthened period of termination. By the first, the patient is induced to go about his ordinary affairs at intervals, until compelled by weakness to take to his bed again; and this occurs sometimes a few hours before he expires: otherwise, the complaint is protracted for an indefinite period, one, two, three, or four months. This fever prevails late in the autumn, and runs through half the winter; its subjects are, persons of broken constitutions, those advanced in years, and women. On the contrary, the remittent and intermittent fevers attack such as have lately arrived, with youthful and vigorous constitutions.

Marsh fevers were unusually prevalent in Upper Canada, during the summer and autumn of 1803. The grenadier company of the 49th regiment, consisting of 75 men including officers, were stationed at a block house at York. It was an advanced post, situated on the low and marshy banks of an almost stagnant river. The consequence was, that every individual of the party was attacked with fever. The post was obliged to be abandoned, nor was it until a removal to a distant part of the country that the men entirely recovered.

During the same season, numbers of people, old settlers round the bay of Quinté, one of the most fertile and populous districts in Upper Canada, died of the lake fever.

The cure of this fever is not less easy and certain at its commencement, than difficult in its advanced stages. An antimonial emetic, followed by a brisk purge, with attention to regimen for two or three days, seldom failed of curing it on the access. But if this was neglected, and the disease far advanced, such a torpor of the system was induced, as frequently rendered ineffectual the most active

tive medicines. In its advanced state, after complete depletion, a remission was best obtained by sudorifics, assisted by blisters and the warm bath; after which the bark was administered in suitable forms; it generally proved inert of itself, unless assisted by aromatics and bitters. The following formula was used with much success in the hospital of Fort George.

℞. Pulv. cort. Peruviani ℥j. R. columbæ, Capsici Cayennensis sing. ʒij. Tere simul ut fiat pulvis. Drachma, ex cyatho vini lusitanic. rubri, quater indies vel sæpius sumenda.

The settlers cure marsh fevers with a fungus or agaric, which they find growing on the beech tree. It is first dried, and then rasped or grated into powder. I tried it by itself, and also joined with the bark, with decided advantage; it is an intense bitter.

From the preceding account of the lake fever, it seems to approximate to the typhus type; indeed, many practitioners of medicine settled in Canada, consider it as a typhus fever. In my opinion, however, it has a character essentially different. It is not contagious, nor did I ever meet with a case in this country of that mixed species of fever (synochus) so common in England.

In the fall of the year 1799, the Asia, a large troop ship, arrived at Quebec from the Cove of Cork, having the 41st regiment and two companies of the 6th on board. The transport was crouded, and not clean; and it was strongly suspected, that some recruits from a prison ship had introduced an infectious fever. Immediately on her arrival at Quebec, the men were transferred from the ship to batteaux in the river, the weather being raw and wet, (the last week of October); from whence they proceeded up the river to Montreal. Scarcely were they settled in quarters, when a malignant putrid fever broke out, exquisitely contagious. Numbers of the men, and several respectable inhabitants, among whom were four medical gentlemen, died of it. Several families removed from the town; and the impression it made was so forcible, that for two years afterwards the inhabitants of Montreal shunned the approach of a soldier.—This event is adduced here, as a proof how little acquainted the inhabitants of these provinces were with contagious diseases.

Quebec, indeed, being a sea port, which carries on a considerable trade with the West Indies, might doubtless be liable, as well as New York and Boston, to the consequences of imported infection. This city is built on a bold

bold promontory, which projects into the river, rising 500 feet above its level. The channel, deep, clear, and rapid, sweeps round three parts of the rock, on whose shelving base the houses of the lower town are crowded, or rather huddled together. It may be easily conceived, that its streets are as filthy as possible. The upper town is more open, but not paved, which defect is partly compensated by the nature of the rock itself; it is composed of a blue schistus, (not of lime stone, as erroneously described) whose strata lie nearly perpendicular with the horizon; the consequence is, that the heaviest rains are soon absorbed.

The climate of Quebec, although in lat. 46° 47′ N. has been compared to that of St. Petersburgh in lat. 60°. but the summer season is more than a month longer. The extremes of heat and cold are, however, equally great.—The population of Quebec is computed to be between 14 and 15,000, including the garrison. Few places can boast of a greater exemption from disease; and it is remarkable, that in a climate at once so variable and severe, disorders of the chest should be of rare occurrence. Strangers, for want of precaution respecting their cloathing at the commencement of the cold season, sometimes fall victims to acute pneumonia; the natives never.

The garrison, until this autumn, enjoyed a similar degree of high health with the inhabitants. The state of the atmosphere in the summer and fall of 1805 was unusually variable; it was throughout wet, and often stormy, with the barometer remarkably low. At intervals the weather became intensely hot, the thermometer ranging during the months of July and August, and in the first week of September, between 84 and 92 deg. Early in September there was a great and sudden change, for the mercury fell to 60 deg. and about the middle of October it was as low as 40 deg. During the greater part of the two latter months, there were continued rains and storms.

During this variable season, there occurred a change of quarters of the troops in both provinces. A detachment of the 49th regiment was doing duty in Quebec; the rest of the regiment was distributed in detachments all over Upper Canada: these, therefore, had to navigate vessels and batteaux six, eight, and twelve hundred miles before they were collected at head quarters. Meanwhile, those troops already stationed at Quebec, underwent severe duty, in consequence of the increasing extent of the works, compared with the smallness of the garrison.

About the middle of August the sick list of the 49th regiment

regiment, which prior to that time contained only five or six trifling cases, had increased to twenty-three, most of them fever cases; and as the detachments from the upper country arrived, it continued to increase, so that about the middle of September the number of sick in hospital amounted to sixty-five. The hospital of the 6th regiment (the greater part of which never left Quebec) was also crouded, although not in the same degree as that of the 49th.

In consequence of this unexpected increase of sick, and of some deaths, the Commandant of the forces in both Canadas ordered a medical board to be formed, to inquire into and report on the nature, cause, and present state of the fever, which then existed in the garrison.

The Board, consisting of all the medical staff officers of the garrison, and of the regimental medical officers, met accordingly; and its Report briefly stated, "That a fever, *accompanied with yellowness of the skin,* was prevalent among the troops in garrison; that this fever did not appear to have been imported, or to be infectious, but that it most probably owed its origin to a season unusually hot and moist, affecting the health of the men during a fatiguing route to Quebec, and of those also doing severe duty in garrison." The Report further stated, "That the fever was on the decline; and that as the cold weather advanced, it was expected that it would altogether disappear."

The access of this fever was sudden and violent; men were seized with it while under arms, and obliged to fall out of the ranks; chills, vertigo, and faintness were immediately succeeded by severe pains in the calves of the legs, in the loins, and in the back part of the head. The prostration of strength was so great, that the patient could not stand without support; the face was in most cases pale and contracted, although in some it was flushed, with inflamed eyes; the tongue was moist and whitish, and as the disease advanced, it was covered with a tough slough of an ash colour; in a few cases it was rough and dry, of a purple colour, with crimson edges. Although the hands felt cold to the touch, yet on pressing the pulse a burning heat might be perceived along the course of the artery; the pulse was tense and vibrated, often sunk and weak, but never quick. Towards the evening of the attack, the violence of the symptoms seemed to abate, and the next morning there was an evident remission. Then, or before the second night, *the tunica conjunctiva might be observed*

tinged

tinged with yellow, which appeared to be spreading from thence to the temples and forehead. On further examination, the neck, axilla, and scrobiculus cordis betrayed also the yellow tinge. The second night proved generally restless, and on the morning of the third day there were severe exacerbations, affecting some patients with furious, others with low delirium, attended with coma. When delirium was absent, there was immense anxiety, with a continued change of posture, vomiting of every thing swallowed, and straining to vomit when there was nothing on the stomach. The matter thrown up, when it did not consist of what was taken, appeared small in quantity, frothy, and pituitous, with sometimes a greenish cast, but it was in no case dark coloured. The vomiting greatly resembled that occasioned by sea sickness. *The whole body now appeared of an intense yellow*, resembling nearest the hue of a fresh ochred wall. There was constipation of the bowels in most cases. Diarrhœa sometimes supervened, when the disease was protracted; it continued obstinate long after the fever had subsided. The urine was made in small quantities; it was of a brick colour, and clouded. In the cases which proved fatal, there was a flattering remission some hours before death; after which the pulse became intermitting; the sphincter muscles relaxed, there was involuntary discharge of the fæces and urine, and the patient calmly expired, or died comatose.

This was the course of the fever, in the greater number of cases; but in many there was considerable difference, as to the duration and succession of symptoms. In five cases there was hæmorrhage from the nose and mouth; two of the five proved fatal. In a female patient (a soldier's wife) the catamenia appeared on the access of the fever, and came on so immoderate, that means were used to restrain it. This patient died on the fourth day from the first attack. James Read, a corporal, a steady, sober man, was the first victim of this fever; towards the evening of the third day he had so complete a remission, that he talked of going to his duty the next morning; he was seized with apoplectic symptoms, however, on that morning, under which he continued in a state of insensibility, with stertorous respiration, for forty hours, and then expired. He had been bled ad ℥xxij on his admission into the hospital.

Of fifty-five cases of this fever, which occurred in the 49th regiment, six only proved fatal, and all these within the

the week. No decisive crisis could be observed in those who recovered. The languor and debility continued for several weeks, and the cuticle retained its yellowness for two months. Relapses of the yellow fever are frequent in the West Indies, but no relapse took place of the fever just described. This was most probably owing to the salutary change which happened in the state of the atmosphere.

In the medical treatment of this disease, the writer of this account was not biassed by any plan of cure which he had seen adopted in the West Indies, nor by any thing he had read respecting the management of the yellow fever in the United States. He considered the fever in question as a malignant species of marsh fever, and had not that apprehension of the exhibition of antimonial emetics, in the commencement of pyrexia, which seems to have influenced, rather too much, the practice of the respectable physicians of the United States. Emetics are dreaded, it seems, as concurring with the disease in increasing the irritability of the stomach. But what remedy is so powerful in restoring the lost tone of that important organ, as the judicious employment of emetics?

In the present instance, depletion by emetics was prefered to the use of the lancet, and a full dose was found more effective than nauseating doses. Antimonii tartarazati, gr. iij. Nitrat. sodæ ℈ij. largely diluted for one dose, seldom failed to operate both ways; it always produced a moist skin and clean tongue, and in numerous instances subdued the disorder in *limine*. But when the disorder was too powerful in its access, the depletion was further pursued. Jalap and calomel in doses of twelve and five grains were administered four or five times in twenty-four hours, or oftener, till the medicine proved effective. But in the majority of cases it was found more advantageous to lose no time, but to throw in a full dose of pulv. jalapii ʒj. calomel, gr. x. ex cyatho aq. menthæ piper. fortis, which seldom failed to procure a copious alvine evacuation. Sometimes however this medicine remained inert in the stomach, in which case it was rendered active by a dose of ol. Ricini.

When the alimentary canal was effectually cleansed, a perfect remission was best obtained by sudorific medicines; and of these the aq. ammonia acetata, in which the volatile alkali was suffered just perceptibly to predominate, was proved to be, by far, the best. This medicine was administered four or five times a day, in doses suited to the state

state of the patient. The diaphoresis was further assisted by the addition of fifteen or twenty drops of sp. ætheris nitrosi, and at night by the further addition of tinct. opii camphor. Bathing the lower extremities in a tepid bath was found very useful.

The most formidable symptom attendant on this fever, and the most difficult to combat, was the incessant vomiting. Great attention was required to watch a temporary cessation of this distressing symptom, and to exhibit cautiously a suitable remedy. Very small doses of a camphorated julep, with a few drops of æther; whilst some was applied externally to the pit of the stomach, frequently succeeded in soothing the perturbations of that exquisitely sensible viscus. But no bulky medicine, even an ounce of liquid; could seldom be retained. A blister applied to the epigastric region was never omitted in urgent cases, and, no doubt, proved servicable. Some of the worst cases was attended with a frightful singultus. Musk, formerly a favourite antispasmodic in fever, was thought of, but could not be procured here.

In addition to the above mode of treatment, a complete ventilation was made through the wards; the nitrous fumigation, and sprinkling the floors with vinegar, were also employed: And the affusion of cold water with vinegar, so far as it could be managed, was never neglected.

The diet, while the symptoms ran high, was very light; it consisted of tea and toast, which was prefered to every thing by the patients; sago, beef tea, &c. The drink was table beer or imperial, made palatable with essence of lemon and sugar. Sometimes cold brandy and water agreed best with the stomach.

On the first decisive remission after the fourth day, (for before then the remissions were illusory) the bark was administered; at first in decoction combined with snake-root, and as the stomach recovered its tone, in substance with an aromatic and a bitter. In the same manner wine was cautiously administered. Many of the patients took a dislike to wine; upon which, strong beer, which is brewed in this place of an excellent quality, was substituted with advantage.

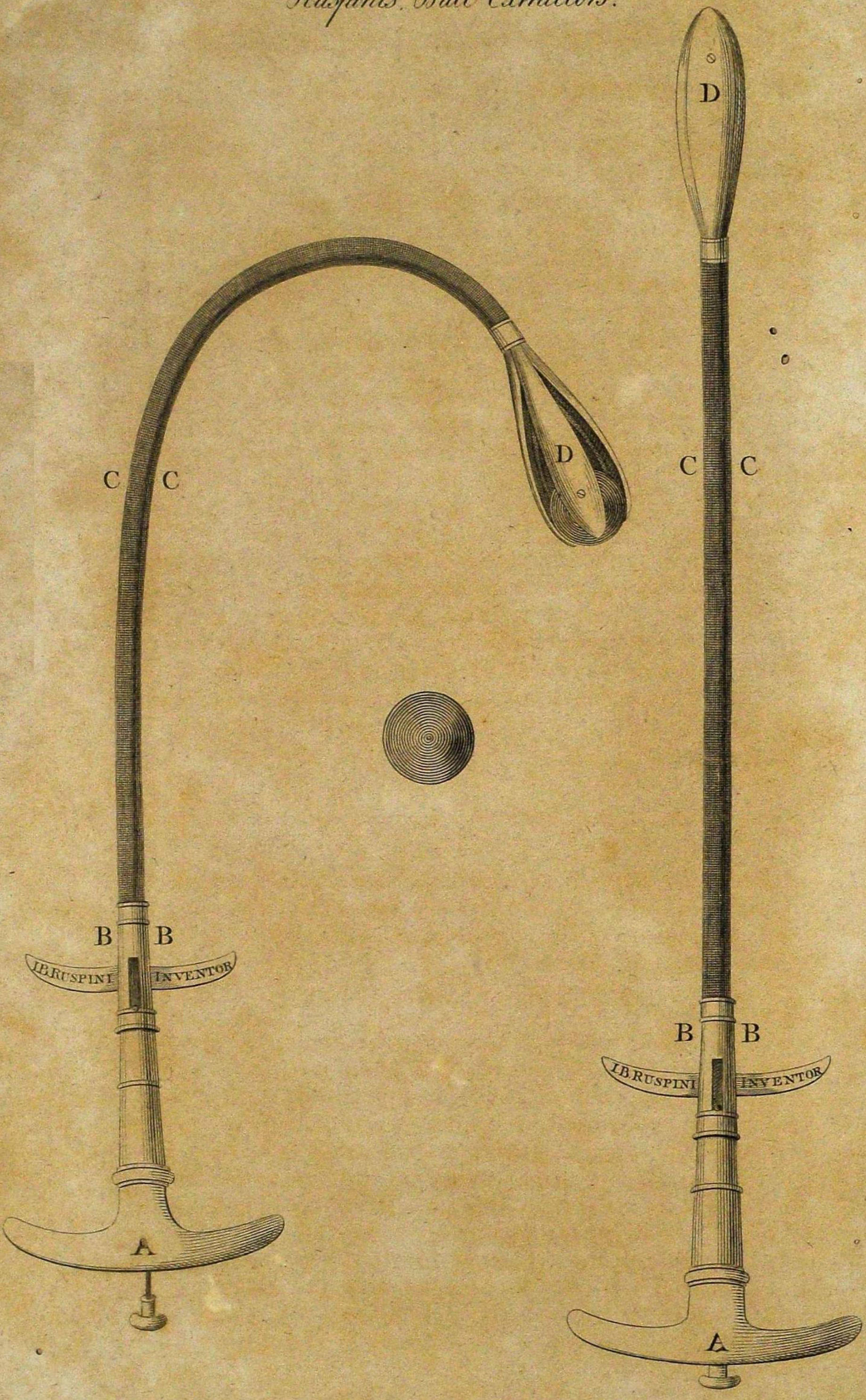
Thus was this formidable and alarming fever subdued, with little comparative mortality.

In concluding this account a very few remarks may be permitted.

It is extraordinary, that the Medical Profession in the United States, and in the West Indies, should still con-

tinue,

Ruspini's Ball Extractors.



tinue, after a dozen years of enquiry and investigation, to be divided in opinion on this important question, viz. whether the yellow fever is an imported, and consequently a contagious disease? or, whether it is of spontaneous origin, and becomes endemic in certain places, from some undescribed change in the constitution of the atmosphere? However this question may be ultimately decided, there can be no doubt, that the fever above described, was neither imported nor contagious; for on the most diligent enquiry, not a single case was found to have happened among the inhabitants of the upper or lower town; and if dirt, that is, animal and vegetable substances, in various stages of putrefaction, be a prime cause of this fatal disease; then, most assuredly, no place in the world is more entitled to it than the lower town of Quebec; but, as already observed, no place of the same extent is, perhaps, so healthy.

Some formidable symptoms of the yellow fever of more southern climates, especially the black vomit, did not take place in this fever, which, on the whole, bore a milder type. It probably differs in degree only from the former, as the slow nervous fever of England differs from the putrid; and it may, therefore, not improperly, be called *typhus icteroides mitior*.

Quebec, Nov. 6, 1805.

A Brief Description of a newly-invented Instrument, for the Extraction of Balls from Gun-Shot Wounds. By J. B. Ruspini, *Surgeon-Dentist to His Royal Highness the Prince of Wales.*

(With Engravings.)

THE enlargement of Gun-shot Wounds, for the purpose of extracting the ball, has been acknowledged by many eminent surgeons to be an evil which, from the nature of the probe and forceps at present in use, it has been hitherto impossible to prevent. Nothing can be more true, as a general proposition, than that the larger the wound, the more difficult it is to cure; whilst by the increased loss of blood, which must unavoidably follow a dilatation of the original wound, the patient is frequently much weakened, and rendered less able to contend with the effects of the injury which he has unhappily sustained. It is not, therefore, an object of small importance, if the means can be devised

devised by which the offending matter may be extracted without the wound being rendered greater in the endeavour to cure it. The probe and the forceps hitherto in use have been found, in many instances, injurious in their operation, as they frequently still further lacerate the part, and often bring away some of the integuments along with the ball. This injury to the patient has been still more seriously felt when the ball has taken an oblique direction. It is not necessary here to enter into any detail of the nature of Gun-shot Wounds, as they have already been much more ably treated upon by others; my only object is to induce those gentlemen who practice as surgeons, to make trial of an instrument, which, from the experiments that have already been made of its utility, appears, I think, calculated to assist materially in the cure of that description of wounds to which I have alluded.

The peculiar properties of the instrument which I have ventured to bring forward for public investigation are, that it acts as a probe as well as forceps, and does not, when used, dilate the wound or lacerate the parts. In the first place, when the ball takes a straight direction, this instrument, on being introduced, discovers the depth of the wound, and immediately on coming into contact with the ball, the secret springs act by opening the claws, which immediately close firmly on the matter to be extracted; by which means it is easily brought out, whilst the parts originally wounded receive no further injury from its action. In the last case, where a ball takes a curved direction, I have prepared an instrument of a similar nature, but flexible, which follows the ball, and, seizing it in like manner, brings it out likewise without further injury to the parts wounded.

Many eminent surgeons have signified their approbation of the instrument, and have declared its superiority to the forceps now in use; and I am convinced the best explanation of the utility of the instrument will be furnished by experience.

The use of the instrument is not, however, confined to Gun-shot Wounds; it will be found of equal utility in Lithotomy and Bronchotomy. I have every reason to believe that it will be found of the greatest use in extracting a stone from the bladder, or any hard substance from the throat; as in either case it will act precisely in the same manner before described, without lacerating the parts affected. In any case, in short, where it is requisite to extract a hard substance, the instrument may be used with a similar effect.

It

It is of no small moment, therefore, to the public, that a fair trial should be made of the instrument, as in the event of its successful application, it may be productive in many cases of incalculable good. In speaking thus, far be it from me to make any peculiar claim to merit on account of the discovery: my only request is, that it may be tried in the fair scale of experiment; in which case, I flatter myself it will frequently be found of considerable utility.

I have purposely avoided entering into a minute description of Gun-shot Wounds, or of those cases in which I conceive this instrument might be found useful, as that would lead me into far too wide a field, and would be in a great measure foreign to the subject of the present pamphlet. I only think it necessary to press upon the attention of my readers, that it is of the greatest importance that all wounds and all diseases, requiring surgical operation, should be cured with as little pain as possible to the patient, and without any unnecessary laceration of the parts, already too deeply affected. It is in this point of view I have ventured to recommend the Instrument which I have invented for general use; and without entering into any detailed comparison of its properties with those of other instruments now employed, I leave it to speak for itself.

In the annexed Plate is a delineation of two Instruments, of which, fig. 1 is to be used when a ball takes a straight direction, or when the substance to be extracted lies in a similar line. A is the handle; BB the secret springs, by means of which the claws open when they come in contact with the ball; CC the straight probe; and D the claws, which close immediately when they have seized the ball or other substance.

Fig. 2, is an Instrument to be employed when the ball has taken a curved direction. A and BB as before; CC a flexible probe; and D the claws, which operate in the same manner as above mentioned, by immediately closing when the ball is within them.

The method of using the Instrument is simply as follows. It is introduced into the orifice of the wound in its closed state, as represented in the straight figure; when it reaches the ball, the points D are expanded by pressing the wings near BB with the middle fingers, backwards toward the handle A, lodged in the palm of the hand, thus opening the points, and by a slight motion forwards of the whole Instrument, embracing and securing the substance to be extracted.

To the Editors of the Medical and Physical Journal.

Gentlemen,

I Am very glad to see in your last number that Dr. Kinglake has taken up the subject of burns; it certainly is a subject of great importance, and demands very serious attention. I wish to preface my reply to Dr. Kinglake by stating, that I have not the slightest knowledge of Doctor Kentish, except from his writings; that before their publication I had too frequently seen the ill effect of cold applications, even that of *cold water,* in burns and scalds, not to wish for a more efficacious mode of treatment; that I do not suppose the *facts* advanced by Dr. Kentish, who appears to have had ample experience in those cases, can be fairly questioned by any one who has not repeated his experiments; and that having seen the happy effect of the treatment recommended by him in the case which I have recorded in your Journal, and which I think was a very serious case, I felt myself justified in giving it to the world.

I am aware that medical facts are by no means easily appreciated, and that therefore it is highly praiseworthy to advance them with modesty, and to reason upon them with care. The laws of excitability are highly curious and interesting, and the Brunonian theory seems to be the first step securely taken towards their developement; but the peculiar sensibilities of particular parts of the body, their relative importance, and their sympathetic influences, demand long and patient investigation. If any man will be hardy enough to reason against facts, I do not see upon what ground he can be met. I *must* suppose that the principal object of a liberal writer is an inquiry after truth. Dr. Kinglake has I think been attacked unfairly; but why should he employ the same unfair weapon? The evidence of a respectable man ought not to be questioned until the experiment has failed in the hands of another; for, after all, experience must decide, and every mode of reasoning must prove futile without its support. I cannot compliment Dr. Kinglake on his analogy of a *runaway horse,* it appears to me to be infinitely more fanciful than just, and to have no other relation to the subject in question than matter has to mind. Dr. Kinglake goes on to remark, that "the theory which prescribes the reconducting of inflammatory disease to the standard of health,

health, by only gradually diminishing the force of the violence which occasioned it, is, in my apprehension, fraught with gangrene and death." But experience has proved that this apprehension, with respect to burns, is groundless; and the Doctor has here evidently confounded the violence of the exciting cause with the violence of the consequent action. As experience has proved the efficacy of the *gradual restoration* to the healthy degree of excitement, from one extremity of the scale of excitability, so has it also from the other. I do not think Dr. Kinglake would be hardy enough to plunge a frozen limb into *hot* or even *warm* water, for experience has most decidedly proved that such a measure would be productive of the "gangrene and death" of the part. And if the Doctor should ride his fiery steed so temperately in a very cold day, as to find when he enters a warm room, his fingers extremely painful; the instantaneous relief afforded by rubbing them with a little snow might serve to convince him, that a very gradual restoration of heat is not only the most agreeable but the best practice; for the too sudden application of heat does not merely occasion great pain, but that pain is followed by a considerable degree of numbness.

Dr. Kinglake then observes, "If, pursuant to the Brunonian doctrine of reducing excitement only by a slow series of diminution, be the correct method of treating diseases of indirect debility induced by excessive stimulation, when can a warranty arise for copious bleeding," &c. &c. and answers the question thus: "under no circumstances whatever." Here the question is again involved in a difficulty, which I confess surprises me. Surely, there is a wide difference between the *transient* application of a powerful exciting cause and one that *continues* to operate with undiminished force. I am disposed to think, that if we had such morbid causes as compleatly within our power as we have that of the external application of heat; by gradually withdrawing them we should cure our patients more speedily, and with less ultimate debility, than by the indirect mode of evacuations. He then says, "the diseases of *direct* debility will imperiously demand liberal excitement." Now this doctrine is any thing rather than Brunonian, for, according to that system, diseases of direct debility are to be cured by the most *gradual* application of stimulants, or, as the author expresses himself, by "gradually wearing down the accumulated excitability." I have said thus much, gentlemen, in support of a theory

 which

which is not my own, but which I think fairly bears me out, and explains the facts I believe better than any other; but as my anxiety is much more to discover and establish truth than to theorize; and as I think that a calm investigation is infinitely better calculated to lead to that discovery, than an intemperate inquiry; if you should perceive any undue asperity in this letter, I beg that you will correct it. I also request Dr. Kinglake to accept my thanks for entering upon the subject.

I am, &c.

Cheshunt, Herts, April 2, 1806. E. HARROLD.

CRITICAL ANALYSIS
OF THE
RECENT PUBLICATIONS
ON THE
DIFFERENT BRANCHES OF PHYSIC, SURGERY, AND MEDICAL PHILOSOPHY.

A Treatiſe on the Origin, Progreſs, Prevention, and Treatment of Conſumption. By John Reid, M. D. Member of the Royal College of Phyſicians, London, Senior Phyſician to the Finſbury Diſpenſary, and Lecturer on the Theory and Practice of Medicine. 8vo. London.

In the Preface to this Work we meet with the following ſtatement of the views with which it was compoſed. "The writer of the enſuing pages has laboured to render the diſquiſitions that they contain, at the ſame time open to general obſervation, and intereſting to the profeſſional ſtudent. He has attempted to delineate a portrait, the prominent character of which ſhall be univerſally underſtood, while the more minute lines and ſhades of expreſſion ſhall remain for detection by the ſkill of the artiſt."

In purſuance of this plan, our author introduces his ſubject with a ſlight ſketch of thoſe different hypotheſes which have, at various periods, been conſtructed in order to explain the nature and origin of what he conceives neceſſary to the actual exiſtence of genuine phthiſis, ulcer in the lungs, and hectic fever. The principal errors of ſuch hypotheſes are attributed, by Dr. Reid, to a neglect on the part of their founders, of regarding life as an effect, produced and ſupported by influences totally diſtinct in their nature from thoſe which regulate the economy of the inanimate creation.

"Theories of diſeaſe were deduced from the principles of looſe analogical

analogical reaſoning; unprofitable diſquiſitions into the cauſe, for a long period, ſupplied the place of an attentive inquiry into the laws and characteriſtic qualities of vitality, and the ſcience of life remained to be diſcloſed."

That reformation, ſo long deſired in the conduct of medical reſearches, has at length been effected, according to the opinion of our author, by the enunciation and application of what is generally denominated the Brunonian theory, the outlines of which are deſcribed by the following words: "Every change which is effected on the ſolids or fluids of the living ſyſtem, is ſubordinate to, and conſequent upon, a certain degree of action in the animate fibre. This action is produced upon principles entirely different from the impulſe of mechaniſm, or the attractions of inanimate matter. It is governed by peculiar and excluſive laws. The powers by which it is generated are the powers by which life is ſuſtained. Health conſiſts in its regular maintenance. The various modifications of diſeaſe are in their origin entirely attributable to its derangement, and death is the inevitable and immediate conſequence of the loſs of ſuſceptibility to its action."

The four ſucceeding chapters are devoted to a delineation of the ſtructure of the reſpiratory organs, an account of the chemical ingredients of atmoſpheric air, and a general inquiry into the functions of the lungs and ſkin, as far as they are connected with the propagation and preſervation of animal heat. Our limits will not permit us to ſcrutinize the reaſonings of Dr. Reid, from which the following inference is deduced, that chemical agency has been too precipitately made to apply to the functions both of the lungs and external ſurface of the body; and that the generation and due adjuſtment of animal temperature, are, like all other phenomena of the living frame, influenced by the peculiar laws of fibrous excitation.

Thoſe affections are next made to paſs under review, which are frequently precurſors of ulcer in the lungs; theſe are treated of in the following order, *Hæmoptyſis, Catarrh, Pneumonia,* and *Tubercles.*

In the chapter on Hæmoptyſis, Dr. R. takes ocaſion to object to thoſe accounts of its origin, which he conceives to be relicks of the mechanical and humoral doctrines in pathology. Diſcharges of blood from the lungs, our author maintains, in no inſtance originate according to the opinion of Dr. Cullen and other writers, from a ſudden diminution of weight in the atmoſphere, acting either upon the principles of mechaniſm, or by occaſioning the circulating fluids to rarify and expand; an effect which he contends is altogether inconſiſtent with the eſtabliſhed laws of vital cauſation. While conſidering the treatment of hæmorrhage from the lungs, as likewiſe the production and treatment of catarrh and pneumonia, the writer's principles are maintained with much ingenuity of argument, and conſiderable force of expreſſion.

We were much gratified with obſerving, that in his remarks on pneumonia, our author adverts, in an eſpecial manner, to the tendency of eruptive diſorders in early life, more eſpecially the mea-

fles, to aſſiſt or even create phthiſical diſpoſition, unleſs immediately and effectually counteracted; for the purpoſe of preventing this occurrence, he is of opinion, with many contemporary writers, that Digitalis furniſhes an important agent.

With reſpect to tubercles, Dr. R. objects to that vague mode of explanation, which refers their origin to conſtitutional acrimony; but does not, in our opinion, ſubſtitute any theory of their production which is explicit or ſatisfactory. It is not ſufficient to deſcribe theſe bodies, as conſequent upon ſlight and repeated inflammation, depoſiting matter which remains unabſorbed. We conceive the queſtion at iſſue to be, Why ſhould that peculiar depoſition, by which a tubercle is conſtituted, reſult from inflammation in parenchymatous ſubſtance excluſively? To aſcribe this to "peculiarity of ſtructure," is not to explain, but merely to ſtate its cauſe.

We are, moreover, diſpoſed to think that Dr. R. has evinced too great a partiality for ſyſtem and generalization, in his endeavours to prove that ſuſceptibility to catarrhal irritation of the mucous membrane, and that the generation of tubercle, exiſt in the ſame ratio. Violent and reiterated inflammation, we are perſuaded, frequently affect the mucous membrane without being followed by tubercle.

In the next diviſion of his work, Dr. R. conſiders the prediſpoſing and exciting cauſe of conſumption; and the characters of the phthiſical temperament are here pointed out with ſo much perſpicuity and accuracy, that we regret our limits do not admit of their detail.

On the means of obviating the conſumptive tendency, we find a conſiderable coincidence of opinion with another modern author of deſerved reputation; a coincidence which will in no meaſure detract from the value of thoſe precepts which are contained in this important diviſion of Dr. R's work.

The progreſſive ſymptoms of phthiſis pulmonalis are traced with fidelity in the enſuing chapter, and the practical rules, with regard to the treatment of the complaint, diſplay much nicety of diſcrimination, and correctneſs of judgment. We conceive, however, that the author would have rendered his treatiſe more acceptable to the majority of readers, by a more ample detail of both the dietetic and medicinal management of the conſumptive invalid. In the earlier ſtages of this complaint, Dr. R. ſpeaks with much confidence of the powers of Digitalis, the modus operandi of which he endeavours to reconcile with Brunonian principles.

The work now under conſideration concludes with additional obſervations on the mode of diſcriminating between genuine and ſpurious phthiſis, with an account of the different ſpecies and cauſes of general decline, and pulmonary conſumption; and with further remarks on the remedies required in conſumptive affections, whether independent of, or connected with, an actual diſorder of the lungs.

We ſhall finiſh our Analyſis by extracting the following paragraphs,

graphs, which terminate the work, and which we think afford a favourable ſpecimen of the general ſtyle in which it is written.

"Coughs are by no means abſolute indications of catarrhal affection. The idea of 'ſpecific remedies for coughs and colds,' is founded upon a total ignorance of the laws and functions of the animal economy. It is equally inconſiſtent with, and contrary to, both philoſophical principles and actual experience, as the vulgar and empirical notion of correcting impurities in the blood.

"Urgent pain is often unaccompanied by inflammation or increaſe of local excitement. It is in many inſtances conſequent upon the difficulty and labour with which an enfeebled organ performs its accuſtomed and ſalutary action. An important demand for invigorating reſources is too commonly ſupplied by enervating powers.

The fluſhed cheek and emaciated countenance, are the moſt faithful attendants upon genuine phthiſis. Even theſe, however, are not abſolute indications either of the preſence or abſence of pulmonary conſumption.

"A familiarity with the phyſiognomy of this diſeaſe can only be acquired by an aſſiduous attention to its ever varying ſhades, and by carefully marking its multifarious expreſſions. In proportion, however, to the difficulty of the taſk is the neceſſity of its accompliſhment. Delays and errors are here eſpecially to be dreaded. Deciſion comes too late, when not merely the nature of the diſorder, but likewiſe its fatal termination, almoſt ceaſes to be a ſubject of doubt or enquiry. To know the rocks on which his veſſel has foundered, affords but ſmall conſolation to the ſhipwrecked mariner."

A Treatise on the Process employed by Nature in suppressing the Hemorrhage from divided Arteries, and on the Use of the Ligature; concluding with Observations on secondary Hemorrhage; the whole deduced from an extensive Series of Experiments, and illusrated by fifteen Plates. By J. F. D. Jones, M. D. Member of the Royal College of Surgeons of London.

We know not whether most to admire the benevolent intentions of this author, his persevering industry, or the accuracy and minuteness of his observation under so long a series of experiments. If ever there was a subject which authorized experiments on brute animals more than another, it is the one before us. It was not with a view of establishing some uncertain theory, some unimportant doctrine, the favourite invention of his brain, but from the sole motive of relieving his fellow creatures under circumstances which admit of no delay, that these enquiries and communications have been made.

That our readers may be enabled to follow us through the rest of the work, we shall transcribe the first chapter, entitled, *preparatory considerations.*

"The changes produced on arteries by accidents, and the surgical

gical operations to which they are subjected, have a certain relation to their structure. The subjects, therefore, of this Treatise naturally suggest a few preliminary observations on the structure of arteries; but only such a description will be given, as may suffice for the explanation of the phænomena which they exhibit, when influenced by accident or art.

"The substance of which arteries are composed is divisible into distinct parts, which have been called tunics or coats. Three coats, which have received various names, can be readily demonstrated, and may be simply and clearly distinguished by the terms —Internal, Middle, and External.

"The internal coat, although extremely thin, is very close in its texture, and gives to an artery a smooth and polished lining; it is elastic, and firm, considering its delicate structure, in the longitudinal direction, *but so weak in the circular as to be very easily torn by the slightest force applied in that direction.* The morbid changes, which have been observed in it, prove that this coat is vascular; and some experiments have been related to show the probability of its being sensible.

"The middle coat, which is the thickest, is formed by numerous layers of firm, compact, fleshy fibres of a pale red colour, passing in a circular direction, but appearing rather obliquely connected and interlaced with each other, than forming complete circles. These fibres are of a peculiar nature, are well supplied with nerves, and resemble in form and disposition, muscular fibres, but differ from them in possessing a remarkable degree of elasticity. Their elasticity keeps an empty and dead artery open and circular; for this coat, when detached from the internal and external coats, still preserves a cylindrical form, whilst they, on the contrary, in a state of separation, become flaccid and collapse. As this coat has no longitudinal fibres, *the circular fibres are held together by a slender connection, which yields readily to any force applied in the circumference of the artery.* The middle coat is intimately connected with the internal and external by very short and fine cellular membrane.

"The external coat, anatomically considered, is so simple, that many authors have thought it sufficient to say, that it is formed of condensed cellular membrane, which becoming gradually of a looser texture, connects the artery with the surrounding parts; but the importance which is attached, in a surgical view, to this coat, renders a more particular account of it highly necessary and interesting. Although ultimately resolvable into cellular membrane, yet it derives from the particular arrangement of its component fibres a characteristic appearance, which distinguishes it from cellular membrane, and entitles it to be ranked as a proper coat of an artery. Internally, or next to the middle coat, its texture is close and smooth, externally more open and rough, in consequence of the cellular membrane by which it is connected with an additional covering. The whole is remarkable for its whiteness

ness, density, and great elasticity. If an artery be surrounded by a tight ligature, its middle and internal coats will be as completely divided by it as they can be by a knife, whilst the external coat remains entire; a fact, which will be commented upon in another part of this treatise, and shewn to be connected with important circumstances. The strength therefore of an artery depends chiefly on its external coat, which answers, in some respects, the purpose of a strong fascia.

"The three coats which I have just described are the only proper coats of an artery; but the different arteries, while in their natural situation, are surrounded with fine cellular membrane that connects them with additional coverings, called sheaths, which are formed within the cavities of the body by the investing membrane peculiar to each, and in other parts by facia, or cellular membrane. The fine cellular membrane, the bond of connection between arteries and their sheaths, varies in length in different parts, and gives an artery a flocculent appearance. If an artery be divided, the divided parts, owing to their elasticity, recede from each other, and the length of the cellular fibres connecting the artery with the sheath, admits of its retracting a certain way within the sheath; an important fact, the application of which will be made in a future part of this Treatise, that describes the natural process by which hemorrhage is stopped.

"In performing the experiments, which will be subsequently related, it was not my intention to investigate the long agitated question respecting the action of arteries, whether it should be ascribed to their elasticity alone, or their elasticity and muscularity conjointly. I have had, however, frequent opportunities, in the course of my experiments, of observing appearances, which incline me strongly to the latter opinion, slightly modified. These appearances made so conspicuous a part of the process, to which my attention was more immediately directed, that I was induced to discuss the subject at some length in my Thesis. Here we are only concerned to notice one important fact relating to the action of arteries, namely: that when an artery is divided, its truncated extremities contract in a greater or less degree, and the contraction is generally if not always permanent. But I have only once seen a distinct contraction, produced by a mechanical stimulus, while the artery remained entire. It happened in an artery on which the effect of galvanism was tried, but it was uncertain, even in this instance, whether the contraction should be referred to the irritation of the wire of the galvanic pile, or to the galvanic influence, which it conveyed.

Arteries are supplied not only with small arteries and veins (the vasa vasorum), but also with absorbents and nerves, and have, in these respects, a similar organization to the other soft parts of the body. This structure makes them susceptible of every change to which living parts are subjected in common; enables them to

inflame

inflame when injured, and to pour out coagulating lymph, by which the injury is repaired, or the tube is permanently closed."

This account, when we consider that the whole of the future experiments are on arteries, is much too short. We admit the caution of our author in not asserting more than he could prove. He observes, with much propriety, that the elasticity of these fibres, which run in a somewhat circular direction, is the means of preserving the cavity of a dead artery: but as he found that the artery had a considerable degree of elasticity, in its longitudinal direction, so as to shorten itself when cut, it became him to define the degree and nature of these elastic powers. By determining how much power remained in a dead artery, and comparing it with the living artery, he might ascertain this as far as the subject would admit; the power lost after death being subtracted from what remained, would show the degree of muscular power, which could only exist with life. What remained must be considered as elastic. It was not necessary, however, to pursue this subject experimentally to any extent. Mr. Hunter's experiments are so numerous, and his accuracy so well known, that a few, to strengthen or confute them, would have been sufficient. The only objection we have to the passage above quoted is, that the author assumes these muscular and elastic powers of arteries, without offering any proofs either from himself, or any other writer. In all other respects, this chapter is highly important, and must be constantly kept in view by the reader, particularly the description of the cellular sheath, and the manner in which a divided artery contracts within it.

The first section is on the process employed by Nature, for suppressing hemorrhage from divided arteries, or, as we should prefer expressing it, on the sources of the constitution, or animal economy, for suppressing, &c.

In the first chapter the author gives a very candid and accurate review of the Theories offered by Petit, Morand, Sharp, Porteau, Gooch, Kirkland, White, and J. Bell. In this we were much pleased to observe the just respect shown to those old French writers, to whom we owe so much; but our obligations to whom are so seldom acknowledged. Mr. Petit, it is observed, perceiving a coagulum lodged at the inside of the extremity of a divided artery, and also that the external part was immersed in a similar substance, concluded that the plug within, and the support without, were the causes of suppressed hemorrhage. M. Morand added to the above causes, the contraction and retraction of divided arteries. The opinions of both these gentlemen, our author, with a becoming candour, shows, were founded on fact, though as he adds, their opportunities did not allow them to pursue their enquiries far enough. Mr. Sharpe's opinion, as expressed in the Edition of 1783, is nearly similar to the above.

Dr. Jones begins his remarks on M. Porteau's account, by regretting how much the value of it is lessened by the severity with which

which his predecessors are treated. M. Porteau, by making similar experiments to those of Petit, found similar results; but in stumps which he had an opportunity of examining at different distances of time, after amputation, he perceived that in some, no coagulum was discoverable within the cavity of the artery, and in others, where coagula appeared, neither its firmness nor adhesion to the vessel, were sufficient to prevent hemorrhage. This induced him to impute the whole to the contraction of the length and corrugation or constriction of the diameter of the artery.

Mr. Gooch's readiness at adopting Porteau's error, as his own discovery, is next delicately touched upon, and also Mr. Kirkland's opinion, that the suppression of hemorrhage was caused solely by the contraction of the arterial diameter; in which he was implicitly followed by Mr. White and Mr. Aikin. But though Mr. White's principal, if not his only concern, should have been with the unassisted powers of the economy, his conclusions are drawn from the condition of a tied artery.

Mr. J. Bell's Theory, being the last that has been offered, and the most likely to interest the rising generation; we shall offer our author's remarks upon it, in his own words.

" The last theory which I have to notice, is that lately published by Mr. J. Bell, who, after freely criticising those which I have already mentioned, confidently asserts, that, " when hemorrhage stops of its own accord, it is neither from the retraction of an artery, nor the constriction of its fibres, nor the formation of clots, but by the cellular substance which surrounds the artery being injected with blood." It is to be regretted, that among the plates which Mr. Bell has very judiciously introduced to illustrate the doctrines of Petit and Pouteau, he has not added one to exemplify his own, which, although it appears to be delivered in very decisive terms, yet, in its affirmative part at least, is vague and inconclusive. It was perhaps on this account that the author subjoined a copious illustration of it, which I shall also quote, that I may not incur the reproach of having given only a partial and unjust view of his theory.

" The stream of blood gradually lessens, because the artery is emptied, and the resistance to the arterial action taken away; the stimulus being gradually lessened, the artery every moment acts less powerfully; and the blood being no longer solicited or urged on by the arterial contractions, forsakes the open artery, and moves along the neighbouring branches. The surgeon claps the point of his finger upon the mouth of the artery, and holds it there; the outward bleeding is prevented; the blood is extravasated into the cellular substance round the mouth of the artery; the cellular substance is slightly injected with blood; that blood coagulates, and that slight barrier is sufficient to restrain the bleeding of a small artery, till the parts inflame, and the artery is entirely stopped.

" Supposing

"Supposing the artery still larger and more powerful, and "that it drives its blood very furiously among the cellular sub-"stance, it is not this slight injection of the cellular substance "that will restrain the bleeding. Whenever the finger is re-"moved the blood bursts through this slight impediment. The "injected cellular substance will not support the artery, unless "the cellular substance itself be also supported. . . .

". . . . . Retraction of the artery has no effect in "suppressing hemorrhagy, but as it fills the cellular substance; and this injection of the cellular substance is but a slight obstacle, "fit to support only the very smallest arteries. The natural "powers which restrain hemorrhagy, do but suppress it for a "time, and expose the patient to secondary hemorrhage."

"According to Mr. Bell's first position it appears, that the injection of blood into the cellular membrane which surrounds the artery, is the only natural cause, by which hemorrhage is stopped; but as he has just before said that it is not stopped by the formation of clots, we are at a loss to know how this injection of the cellular membrane effects the suppression of hemorrhage. If we have recourse to his explanation, our curiosity is not gratified, and our embarrassment certainly not diminished, for we there find that the cellular membrane is only "slightly injected with "blood," so that we are not at liberty to infer that it is so completely injected as to compress the artery, and in that way stop the flow of blood. Indeed, however plausible such an assertion might have made the doctrine, it would not have been valid: but, continues the author, "that blood coagulates, and "that slight barrier is sufficient to restrain the bleeding of a small "artery, &c." But what is the nature of this barrier? and in what manner does it stop the hemorrhage? for it is to be remembered that we are previously informed, in positive terms, that it is not stopped by the formation of clots; and although, in conformity with this, it is not said that the blood is effused into the cellular membrane, *at the mouth* of the artery, but round it, yet from what has been said above, it must, I think, be pretty evident that an effectual compression of the artery is not implied.

"Left then, as we have been by the author, to make the best we can of the theory and explanation, the only conclusion at which we can arrive without assistance from him, is, that hemorrhage is not stopped "by the formation of clots, but by the cellular substance, which surrounds the artery, being injected with blood, which, according to the illustration, coagulates;" but as the artery is every where surrounded with cellular substance, the coagulated blood, which stops the hemorrhage, must necessarily be in the cellular substance: we have, therefore, only to discuss the difference between a clot of blood and coagulated blood, to discover the principal difference between this offspring of Mr. Bell, and what he has been pleased to call Petit's "sickly child."

"Retraction of the artery," says Mr. Bell, "has no effect in suppressing

suppressing hemorrhagy, but as it fills the cellular substance." But let it be observed, that besides filling the cellular substance round the artery, it also fills the cellular substance at the mouth of the artery in a particular manner; for the divided artery, by its retraction within its cellular sheath, leaves a space of a determinate form, which, all the circumstances necessary for the suppression of hemorrhage operating, is gradually filled up by a distinct clot. If Mr. Bell really means to confine his doctrine of the natural means of suppressing hemorrhage to the injection of the cellular membrane round the artery with blood; he dwells improperly on one of the attendant circumstances to the exclusion of the retraction and contraction of an artery, and the formation of a distinct clot—all of them primary means in the natural suppression of hemorrhage, of which abundant proofs will be given in the proper place.

"I have chosen to set Mr. Bell's theory in its strongest and best point of view, by confining it to the injection of the cellular membrane with blood; for, in justice to his physiological knowledge, I think it unnecessary to examine the accuracy of the reasoning, by which he would shew, that the blood *forsakes* the *open* artery; rather supposing, that, on this occasion, he has hastily adopted the account given by the Editor of the Memoires de l'Academie Royale des Sciences for the year 1735; who, in giving an abstract of Petit's paper, for the year 1731, says, "Dans le cas d'un tronc d'artere coupé, *le sang qui continue de s'y rendre, ne doit plus y couler que jusqu'a l'endroit, ou il rencontrera une branche collaterale entiere dont il enfilera la route*, au moyen de quoi la circulation s'achevera." And I am still less disposed to dwell on the inconsistency of introducing the surgeon's finger as an auxillary to the *natural* means by which hemorrhage is stopped."

After this account of the labours of his predecessors, our author begins the history of his "experiments on the arteries of dogs and horses, to ascertain the process, and the order of the events which constitute it." We shall not enter into a detail of these at present, reserving our remarks on some particular passages to the conclusion drawn from the experiments. Suffice it to say, that equal industry and judgment are shown in every part; that the result is perfectly satisfactory; that if there is any thing to which we object, it is, adopting Dr. Jones's own expression, "to the manner of accounting for the appearances which he observed." However, as we cannot add, that "his doctrine could only have been deduced from the irregular and partial observations which he was enabled to make," we would not wish that our objections should be considered in any other light than as opinions, which the reader will receive or reject as he pleases.

"The results (says our author) of the experiments related in the last section will not allow us to give so conscise and simple an account of the process, as has hitherto been done; but they afford

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us one more satisfactory, becauc it accords better with the operations of the animal œconomy, in which we are accustomed to observe the most important changes gradually produced by the co-operation of several means, rather than by the sole influence of any one in particular.

"They accordingly shew, that the blood, the action and even the structure of arteries, their sheath, and the cellular substance connecting them with it; in short, that all the parts concerned in or affected by hemorrhage, contribute to arrest its fatal progress, by operating, in the case of a divided artery of moderate size, in the following manner.

"An impetuous flow of blood, a sudden and forcible retraction of the artery within its sheath, and a slight contraction of its extremity, are the immediate and almost simultaneous effects of its division. The natural impulse, however, with which the blood is driven on, in some measure counteracts the retraction, and resists the contraction of the artery. The blood is effused into the cellular substance between the artery and its sheath, and passing through that canal of the sheath which had been formed by the retraction of the artery, flows freely externally, or is extravasated into the surrounding cellular membrane, in proportion to the open or confined state of the external wound. The retracting artery leaves the internal surface of the sheath uneven by lacerating or stretching the cellular fibres that connected them. These fibres entangle the blood as it flows, and thus the foundation is laid for the formation of a coagulum at the mouth of the artery, and which appears to be completed by the blood, as it passes through this canal of the sheath, gradually adhering and coagulating around its internal surface, till it completely fills it up from the circumference to the centre.

"A certain degree of obstruction to the hemorrhage, which results from the effusion of blood into the surrounding cellular membrane, and between the artery and its sheath, but particularly the diminished force and velocity of the circulation, occasioned by the hemorrhage, and the speedy coagulation of the blood, which is a well known consequence of such diminished action of the vascular system, most essentially contribute to the accomplishment of this important and desirable effect.

"A coagulum then, formed at the mouth of the artery, and within its sheath, and which I have distinguished in the experiments by the name of the external coagulum, presents the first complete barrier to the effusion of blood. This coagulum, viewed externally, appears like a continuation of the artery, but on cutting open the artery, its termination can be distinctly seen with the coagulum completely shutting up its mouth, and inclosed in its sheath.

"The mouth of the artery being no longer pervious, nor a collateral branch very near it, the blood just within it is at rest, coagulates, and forms, in general, a slender conical coagulum, which

which neither fills up the canal of the artery, nor adheres to its sides, except by a small portion of the circumference of its base, which lies near the extremity of the vessel. This coagulum is distinct from the former, and I have called it the internal coagulum.

" *In the mean time the cut extremity of the artery inflames, and the vasa vasorum pours out lymph, which is prevented from escaping by the external coagulum. This lymph fills up the extremity of the artery, is situated between the internal and external coagula of blood, is somewhat intermingled with them, or adheres to them, and is firmly united all round to the internal coat of the artery.*

" The permanent suppression of the hemorrhage chiefly depends on this coagulum of lymph; but while it is forming within, the extremity of the artery is farther secured by a gradual contraction which it undergoes, and by an effusion of lymph between its tunics, and into the cellular membrane surrounding it; in consequence of which these parts become thickened, and so completely incorporated with each other, that it is impossible to distinguish one from the other: thus, not only is the canal of the artery obliterated, but its extremity also is completely effaced, and blended with the surrounding parts.

" When the wound in the integuments is not healed by the first intention, coagulating lymph, which is soon effused, not only attaches the artery firmly to the subjacent and lateral parts, but also gives it a new covering, and completely excludes it from the external wound, which then goes on to fill up and heal in the usual manner.

" The circumstances now described are observed also in the inferior portion of the artery, or that which is supplied with blood by anastomosis; with this difference only, that its orifice is generally more contracted, and the external coagulum is much smaller than the one which adheres to the mouth of the superior portion of the artery, or that from which the blood flows in its direct course from the heart.

" From this view of the subject we can no longer consider the suppression of hemorrhage as a simple or mere mechanical effect, but as a process performed by the concurrent and sucessive operations of many causes: these may briefly be stated to consist in the retraction and contraction of the artery; the formation of a coagulum at its mouth; the inflammation and consolidation of its extremity by an effusion of coagulating lymph within its canal, between its tunics and in the cellular substance surrounding it.

" And we may conclude that, except in some rare instances, in which the strong retraction and contraction of a divided or lacerated artery prevents hemorrhage altogether, a languid state of the circulation is necessary for the accomplishment of the natural means by which the hemorrhage is stopped. These means may be divided into the *temporary* and *permanent:* under the former head we may include the three first of the above-mentioned causes;

causes; whilst the effusion of lymph constitutes the permanent: yet even these can be distinctly traced only for a certain time, in consequence of other changes which the artery gradually undergoes. Its obliterated extremity no longer allowing the blood to circulate through it, the portion which lies between it and the first lateral branch is no more distended and excited to action as formerly; but gradually contracts, till at length its cavity is completely obliterated, and its condensed tunics assume a ligamentous appearance. At the same time, the remarkable appearances at the extremity of the artery are undergoing a considerable change; the external coagulum of blood, which in the first instance had stopped the hemorrhage, is absorbed in the course of a few days, and the coagulating lymph, which had been effused around it, and had produced a thickened and almost cartilaginous appearance in the parts, is gradually removed, and they again appear more or less completely restored to their cellular texture.

"Nor are these all the changes which the artery undergoes; for, if examined at a still later period, the ligamentous portion is found to be reduced to a filamentous state, distinguishable from the surrounding cellular membrane only by being somewhat coarser; and thus the obstruction which commenced at the extremity of the canal terminates in the complete annihilation of the artery to the first lateral branch.

"But long before this final change is accomplished, many of the lateral branches of the superior and inferior portions of the artery have become very much enlarged, and have established by frequent anastomoses, a free and ready communication between these disunited parts of the trunk. The small branches by whose immediate inosculation these anastomoses are formed, appear to have undergone the principal changes; they are not only proportionably more enlarged than the large branches of the limb to which they belong, and very considerably larger than the corresponding branches of the other limb, but have also become longer, and, being confined within their former space, assume a beautiful tortuous and serpentine course, in order to accommodate themselves to it.

"The circulation appears to be carried on as perfectly and vigorously by these anastomising branches in the limb, the main artery of which has been divided, as in that in which the artery is entire; the inferior part of the divided artery, and all its branches, being found fully equal in size to the corresponding part of the trunk and branches of the artery of the opposite limb which has not been divided: and hence, we may conclude, with the celebrated Mr. Hunter, that "Vessels have a power of "increase within themselves, both in diameter and in length, "which is according to the necessity, whether natural or dis-"eased."

"I shall now make some further observations relative to the external

external and internal coagula of blood, and the intermediate one of lymph, which, if introduced before, would have too much interrupted the detail of the process. And first, of the external coagulum—

"Its particular figure and extent vary according to the manner in which the wound has been inflicted. If the artery, its sheath, the vein, and nerve accompanying it, have all been completely divided, the figure and extent of the coagulum will depend on the relative retraction of the artery to that of the sheath, which varies in different animals, and according as the artery has been more or less detached from the surrounding cellular substance.

"If the *artery alone* be divided, the anterior part of its sheath having been opened longitudinally, the coagulum which then forms at the mouth of the artery varies from a quarter to near half an inch in length, and differs from the form of the artery only in being sometimes slightly conical at its extremity; which, when the integuments have been sewed up previous to the division of the vessel, is turned forward, and is continuous with a large globular portion of coagulum, which being confined by the integuments alone, lies over it and the extremity of the artery. This appearance of the external coagulum may readily be accounted for from the circumstance of the sheath not having been divided so as to admit of its retraction; whereas, the complete division of the artery allows it to retract even within the part to which the wound of the sheath has extended, and, accordingly, that portion of the coagulum which lies within the entire part of the sheath, assumes the form of the artery; while the portion of sheath which has been opened, being deprived of the support of the artery, and of its tension, is more or less disposed to collapse, and will necessarily give a conical appearance to the portion of coagulum formed within it; and thus it is that the lower extremity of the external coagulum is sometimes conical: but when the integuments have been sewed up previous to the division of the artery, in consequence of the impediment which that circumstance affords to the exit of the blood, even this flaccid portion of the sheath becomes fully distended with blood, and of course the conical form of the external coagulum is prevented.—I have been thus particular in explaining these circumstances, because I wish it to be clearly understood, how a slight difference in the manner of performing the experiments may occasion a variation in the appearance of the parts when examined; and if, in repeating experiments on this subject, any deviation from the account here given should be observed, I am convinced it will be satisfactorily accounted for, by attending to the manner in which the experiment is performed in both instances.

"The extent, and, indeed, even the formation of the *internal* coagulum of blood, depends very much on the distance from a lateral branch, at which the division of the artery has taken place;

place: thus, if it is divided about a quarter of an inch beyond a branch, there will scarcely be formed any coagulum of this description, for the effusion of lymph at the cut extremity of the artery is in general sufficient to form a coagulum which extends a little way within the artery, and then the space between the extremity of the coagulum of lymph and the lateral branch is so short, that no internal coagulum of blood can be formed, at least none worth mentioning. In some instances I have found a small lamina of coagulated blood, not thicker than a six-pence, lying on the coagulum of lymph, the extremity of which in these cases generally projects a little beyond the extremity of the artery, and extends further within its canal than merely the surface by which it adheres; which circumstance seems to depend on a larger quantity of lymph being effused than is necessary to fill up the canal of the artery as far as the inflammation extends on its internal surface, and the superfluous quantity not coming in contact with an inflamed surface, and the blood being constantly driven between it and the sides of the artery, it forms no adhesion, but projects a little within the canal. It is probable that the compression, which the lymph undergoes from the gradual contraction of the extremity of the artery, may also contribute to this effect.

"But when the division of an artery has taken place at some distance from a lateral branch, a long conical internal coagulum is then formed, whose base is situated towards the extremity of the artery, and in general it adheres partially at the circumference of its base to the internal surface of the artery, close to the coagulum of the lymph.

"The internal coagulum of blood, however, does not fill up the cavity of the artery throughout the whole of its extent; and though conical, it has often the appearance of not having been formed at once, but by the successive coagulation of small quantities of blood. It is very readily distinguished from the coagulum formed by the effusion of lymph from the inflamed extremity of the artery, which is rather brown than white at first, probably in consequence of some admixture of red particles, and which is *principally characterized by adhering almost throughout its whole extent to the internal surface of the artery*: whereas the former, *i. e.* the internal coagulum of blood, although much longer than this of lymph, forms no adhesion whatever, except a slight one at its base, and which seems to be produced by the coagulum of blood being formed while the lymph is sufficiently recent to allow it to stick to it. Although the internal coagulum of blood when first formed by no means fills up the canal of the artery, except at its base; yet, in consequence of the contraction which the portion of artery containing it gradually undergoes, after a short time it embraces the coagulum so closely that they appear to cohere to each other; so that although the greater part of the coagulum, preserving its natural form, may very easily be

be separated from the artery, yet its internal surface is left of a black colour, as if an external lamina of the coagulum still remained on it. It is also highly worthy of notice, that on examining, at a distant period after these experiments, arteries which, (from similar experiments on the corresponding arteries of other animals more speedily examined), we know must have had considerable coagula of blood in them; no coagula should be found in them; but their internal surface is very black, and their external appearance, previous to being cut open, remarkably dark.

"From what has been said it appears, that when an artery has been divided at some distance from a lateral branch, three coagula are formed: one of blood, externally, which shuts up its mouth; one of lymph, just within the extremity of its canal; and one of blood, within its cavity, and contiguous to that of lymph.

"I have called that of lymph a coagulum, becauſe, when the divided artery has been left entirely to itſelf, there is ſuch a quantity of lymph effuſed, that although it is firmly united to the internal ſurface of the artery, it may be conſidered as a diſtinct ſubstance; but, if the cut edges of the extremity of the artery had been kept in contact with each other by pressure, they would have cicatrized, and no coagulum would have been formed; *i. e.* coagulating lymph would not have been effused in such a quantity as to form a mass of a determinate figure.

"I have already remarked, that when the division of an artery has been made very near to a lateral branch, no internal coagulum of blood is formed: hence we see that the number of coagula varies according to circumstances. But the external coagulum is always formed, and is subject to no other variations than those already deſcribed.

"The internal coagulum of blood contributes nothing to the suppression of hemorrhage in ordinary accidents, because its formation is uncertain, or when formed it rarely fills the canal of the artery, or, if it fills the canal, does not adhere to the internal coat of the artery. Hitherto, therefore, I have contented myſelf with noticing its existence, or pointing out the circumstance which prevents its formation, without ranking it amongst the means which Nature employs for the suppression of hemorrhage. But if an artery be lacerated, its internal coat will be torn in many places, in proportion to the degree of violence with which the injury has been inflicted. Under this particular accident the internal coagulum of blood may extend beyond many collateral branches, will fill the canal of the artery, and will adhere to its surface wherever it is lacerated, in consequence of lymph being effused from these several wounds of the internal coat. The internal coagulum may in this case avail against a return of hemorrhage."

The experiments, without doubt, bear our author through all the proceſſes he has remarked, and even thro' the cauſes to which he aſſigns thoſe proceſſes. But it is not enough to ſhow, that coa-

gulum on the part external to the orifice of the divided artery is the immediate and temporary means of ſuppreſſing the hemorrhage, or that the coagulum within the orifice is, for the moſt, accidental or unneceſſary, or even that the future and permanent ſuppreſſion of the hemorrhage ariſes from the effuſion of coagulable lymph at the divided extremities of the internal coat of the veſſels. If our Readers (and we are ſure all who are worth the labour we have willingly beſtowed on this article will) take the trouble of re-peruſing our extracts, as far as the quotation from Mr. Hunter, they will enjoy a ſecond time the deſcription given by our author of the changes produced in conſequence of ſo ſerious a violence as the diviſion of a conſiderable artery; and they will alſo feel, as the author ſeems to us to have done, not perfectly ſatisfied with the cauſes he aſſigns for ſome of the operations he obſerved.

The firſt effect of ſuch an injury is the coagulation of the blood to ſtop the orifice. This the author ſeems to impute (we admit with ſome caution) to the diminiſhed force of the circulation. We ſhould rather impute it to the reſources of the economy to preſerve itſelf under certain circumſtances. We alſo admit, that theſe reſources are not always ſufficient; but ſuch is the caſe in many other curative intentions of Nature. We well know that the bare ſtagnation of blood, either within or without the veſſels, provided it is ſtill encloſed within living animal matter, is not ſufficient to produce coagulation. This is often proved by large portions of blood in a grumous ſtate, as it is called, which ſometimes forms the ſubſtance of tumours ariſing probably from ruptured veins. Mr. Hunter gives ſeveral other inſtances, in which blood, both extravaſated in the living body, and remaining ſtagnated in its proper veſſels, did not coagulate. He alſo ſhows other inſtances, in which coagulation took place much higher up in the arteries, than the ſpace between the obſtructed part and any collateral branch. In this latter caſe, mortification had commenced at the lower extremity, and was ſpreading upward. A *neceſſary* conſequence of mortification would be a ſeparation of the mortified part and a conſequent hemorrhage. To prevent this, the blood coagulates in the veſſels. When an artery is punctured, ſo as to form what is called the ſpurious aneuriſm, or when the coats of an artery give way in the true aneuriſm, coagulum is formed over the injured or divided part, and, in ſome inſtances, has been found ſufficient to remove the diſeaſe by the obliteration even of a conſiderable veſſel.

As, therefore, in ſo many inſtances, coagulation does not follow extravaſation where it would be injurious to the animal, and does take place in ſo many others where it is abſolutely neceſſary for the preſervation of the animal's life; are we not authorized to ſay that it depends on the neceſſity the parts are under for the ſupport of life, and ſeems regulated by it? Are we not authorized in adopting Mr. Hunter's language, and conſidering coagulation in theſe inſtances, as *an action of the blood induced by the ſtimulus of neceſſity*

ceſſity in the ſurrounding parts? In one of our author's experiments, he found that a tea cup full of blood taken immediately after the inciſion into the carotid of a horſe, coagulated in five minutes and a few ſeconds; the ſame quantity, taken a quarter of an hour after, when the *neceſſity* became more urgent, as the animal was much enfeebled, coagulated in three minutes and a half. This ſpeedier coagulation could not ariſe from any diſpoſition to inflammation from greater powers in the animal; but from an ineffectual attempt at ſuppreſſing the hemorrhage; an attempt, however, which might have been ſucceſsful, had all the other reſources been allowed their full effect. We are told, the ſtream of blood had diminiſhed; the horſe fell; ſhowed great ſigns of reſtleſsneſs; and, to prevent miſchief, was diſpatched when in articulo mortis.

The quotation from Mr. Hunter is beautifully illuſtrated by the picture contained in our author's deſcription. We mean not to ſpeak of this enlargement of the collateral branches of an obliterated veſſel as a diſcovery of late times; it muſt have been known as long as the deſtruction of an artery was known. But we wiſh our readers to remark, that this altered action can only be imputed to the ſame ſtimulus of neceſſity, and is in perfect harmony with every other proceſs we perceive in the blood and the veſſels; that is, the parts act according to a neceſſity which ariſes for preſerving the whole animal, ſo that we may, without any ſtrain of words, be allowed to impute ſuch actions to the laws conſtituted for this particular ſtate of the animal.

The copious extracts we have already made, and the cloſe reaſoning required to accompany us through theſe obſervations, induce us, at preſent, reluctantly to attend to the limits of our work, and to the patience of our readers. In the next number the ſubject will be concluded.

An Eſſay on the Effects of Carbonate of Iron upon Cancer, with an Inquiry into the Nature of that Diſeaſe. By RICHARD CARMICHAEL, Member of the Royal College of Surgeons, and Surgeon to St. George's Hoſpital and Diſpenſary. 8vo. 106 pages. 1806.

THIS work begins with a complaint on the *ennui*, occaſioned by the peruſal of modern medical productions For our own parts, we find, with a very few exceptions, ſtill greater *ennui* in the peruſal of the ancients. To relieve his reader, however, Mr. Carmichael begins, by relating his caſes: theſe are certainly very ſtriking, and ſo well worth recording, that we ſometimes wiſh the author had reſerved his ſubſequent reaſoning till he had more completely matured his theory. The caſes are five in number. The firſt is of a woman who had a foul ulcer on each ſide of her noſe, ſimilar to the deſcription uſually given of *noli me tangere*; theſe were healed by the internal uſe of cicuta and calomel and the ſprinkling of hydrargyrus muriatus ruber on the parts; but in about two months afterwards, a foul ulcer

ulcer appeared on the lip, which could not be relieved by the same remedies. This induced the author to consult the modern writers on the subject. By some accident he was directed to Dr. Adams's work, which our readers will recollect is confined to the cancerous breast.

"The grounds on which Doctor Adams founded his arguments in favour of the independent life of Cancer, seemed to me not unreasonable, although between that disease and Hydatids, I could not perceive any very great similarity. But this obviously led me to the consideration, that if the lives of those supposed animals were extinguished, they would be expelled from the body by suppuration, — and as iron has been found to be very effectual in destroying intestinal worms, I was induced to hope, that it would be equally destructive to other animals of a parasitical nature.—I, therefore, felt myself justified in making trial of a medicine in itself harmless, the effects of which more than answered my expectations."

By the history of the case, it appears not only that the carbonate of iron was successful, but also that as often as that remedy was omitted, by any accident, before the compleat cicatrization of the sore, the ulcer constantly spread, and became ill-conditioned. During the cure, we are told, the ulcer occasionally discharged substances about the size of the smallest pea.

The second case was a cancerous ulcer in the external canthus of the eye; this was healed by the same remedies, carbonate of iron: and in the progress, the author remarked "small cavities resembling those greater ones observable in issues after the removal of the pea." This appearance, he concludes, was owing to the discharge of small bodies, similar to those mentioned in the former case.

The third case was an ulcer of that *species*, called *Noli me tangere*, in a very young lady. Before we proceed any further, we cannot help confessing, that we have never met with this disease in a very young subject. However, the carbonate of iron cured the complaint in six days, which had withstood, for as many years, the most *powerful* remedies. This ulcer does not appear to have discharged any of those round substances.

The fourth case was of a man, forty years of age, with cancerous ulcers on the scrotum and on the calf of the leg, the latter covered with a number of warty excrescences. These were all discharged by the carbonate of iron, and both the ulcers yielded to the use of that remedy applied in various ways.

The fifth case was of a soldier nearly eighty years of age.—Here also the remedy was successful; but it will easily be conceived the patient did not long outlive the cure of his local complaints.

After the relation of his Cases, Mr. Carmichael introduces his reasoning by an account of what has been done by the ancients. This begins with Hippocrates, and is pursued as far down as Helvetius. Whether all the intermediate writers are included, we did not think it necessary to enquire, considering how little was to be learned from those who are quoted.

In

In reviewing the moderns, we are informed of Boerhaave's inspiſſating, Heiſter's coagulating theory, Le Dran's and Pearſon's obſtructions. We think our author too ſevere on Darwin, whoſe hiſtory of the formation of Scirrhus and Cancer is certainly not bad for a poet.

"But," ſays our author, "an hypotheſis has been lately revived by Dr. Adams, which aſſigns to cancer a life independent of the part in which it is ſituated." It is well known that Dr. A. aſſigns to carcinoma that ſpecies of life which has been detected in hydatids. We, therefore, expected to have learned who are the writers from whom Dr. A. received thoſe opinions, which he is ſaid to have revived. Inſtead of this, we are introduced to the ſmall worms mentioned in the Bibliotheca Anatomica; the inſects which Juſtamond conceives, might be admitted at the external pores of the breaſt, and which ſome of the French ſurgeons *had ſeen.* All this is ſurely very different from the nature of hydatids.

Many other remarks follow, which evidently prove that Mr. Carmichael has only an imperfect knowledge of Dr. Adams's Theory; however, in the midſt of all this, we meet with one important objection. "Theſe appearances," ſays the author, "ſeem to be merely the effects of the deranged animal economy, excepting the one evincing a contractile power, in what he (Dr. A.) calls the capſules, but which, notwithſtanding repeated inveſtigations, I never could perceive. The colour of the fat, which Dr. A. ſeems to lay ſo much ſtreſs upon, may probably be produced by animal hepatic air, which Dr. Crawford has proved to be "capable of imparting to the fat of recently killed animals a green colour;" and that this very air, united to ammonia, eſcapes in great abundance from the cancerous as well as other malignant ulcers." The colour of the fat we do not recollect that Dr. A. lays ſo much ſtreſs on; becauſe he remarks, that when cold, it has all the properties of common fat. But if Mr. Carmichael has carefully inveſtigated that ſubſtance when recently taken from a cancerous breaſt, and found none of the papillary appearance mentioned by Dr. A. he is certainly authorized in rejecting the theory.

"As to the cells in which this ſubſtance is depoſited, and which ſeem ſo ſtrong, that without breaking them, their contents may be dug out; they appeared to me on diſſection, to be common cellular membrane, partially ſupported by the ligamentous bands, or what he terms the fungus, but which had leſs the appearance of loculamenta containing a liquid, than the roots of a plant penetrating where there was leaſt reſiſtance — but with every attention, I could not diſcover the papillary appearance from which Doctor Adams deduces the proofs of the contraction and the evidence of the life Cancer.—But if any motion ſhould be obſerved by future inveſtigators, the vitality may be attributed with as much reaſon to the cartilaginous as to the pituitous ſubſtance."

Here alſo, if Mr. Carmichael, in attempting to remove the fat from theſe cells, found it ſo univerſally enveloped with the common cellular ſubſtance, that he could not ſeparate it without break-

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ing the cells as in common fat, he is fairly at issue with Dr. A. and the controversy must remain open for future enquiry.

The fifth chapter contains "Enquiries into the Nature of Cancer." Here our author again begins with Dr. Adams, whose opinions, he observes, are, notwithstanding the objections he has made, consonant with his own, as to the independent life of cancer, though he differs as to the seat of that life. We most sincerely wish he had followed a plan, first, we believe, introduced by Dr. A. and since adopted by most succeeding writers on this subject, that is, not only to tell us what he means by cancer, but also to make some division, according to the different structures of diseased parts, generally considered cancerous. It is remarkable, that none of the cases he gives seem to have the smallest analogy with that which is the subject of Dr. Adams's treatise; yet in his enquiry he seems to include every thing that has ever been denominated cancer; and in so doing, has no difficulty in proving the separate vitality of cancer.

"The origin of carcinoma, says Mr. C. first commencing in a point; the formation of cysts in its texture; these cysts evincing a contractile power by a forceable expulsion of their contents on being punctured; and the peculiar pain in this disease, are all circumstances which strongly impress the idea that carcinoma is possessed of individual life.

"The force with which the fluid is expelled from the cysts, is undoubtedly far greater than the simple elasticity of the part is capable of effecting and it is not a little surprising that some deduction has not been already drawn from this circumstance: but as those who have not witnessed the fact, will be pleased with a corroboration of my assertion, shall recite a passage from Le Dran, who, endeavoring to demonstrate the infectious nature of cancer, accidently mentions a strong proof the contractile power of those cysts. He relates, "that in the middle of a carcinomatous tumour "extirpated by his father, there was a cyst filled with a fluid, "which he opened, and part of its contents *spurting out* upon his "clothes, destroyed the colour of them, as if it had been aqua-"fortis; some of it *flew in his face*, and he felt continued shootings "there for several hours, though he immediately washed the part." Therefore the strong force with which these cysts expel their contents, is not owing to their excessive repletion, but entirely to the action of fibres endued with a contractile power. Such a power, even in a smaller, degree is esteemed sufficient to establish the animality of hydatids generated in the human body, and on the same grounds the vitality of Cancer may likewise be inferred."

It would have been surprising indeed, if Dr. Adams had overlooked this circumstance in supporting his favourite hypothesis. The mention of muscular power in cysts, containing fluids of various descriptions, and their frequent occurrence in parts which are found the most favourable nidus for what he calls carcinomatous hydatids, occurs in several parts of Dr. A's. treatise. However, we trust that gentleman will feel the obligations he owes Mr. Carmichael for this quotation from Le Dran.

The

The ſucceeding chapter is on the treatment of cancer, in the courſe of which the author ſeems willing to believe that iron is injurious to all animals whoſe blood is without red particles. When we conſider how univerſally this mineral is diffuſed throughout nature, we cannot but heſitate before we admit of ſuch a concluſion. If it really is the caſe that iron deſtroys ſuch animals as are without red blood, we ſhall be in poſſeſſion of a remedy more valuable than we need expatiate on. Independent of all our doubts concerning the carcinomatous diſpoſition in the caſes deſcribed by our author, the remedy appears to be a very valuable one, and we truſt, by degrees, its full merits will be appreciated.

A Poſtſcript follows, in which more caſes are given on the authority of Dr. C. and his brother practitioners; though they are ot ſufficiently numerous to eſtabliſh the credit of his remedy in ſo ſtubborn a diſeaſe, we cannot conclude without acknowledging our obligation to the author for his communication.

A Practical Treatiſe on various Diſeaſes of the Abdominal Viſcera. By C. R. Pemberton, M. D. F. R. S. Fellow of the College of Phyſicians, &c. &c. 8vo. pp. 180. London, 1806.

Works on ſubjects of medicine or ſurgery, which embrace a conſiderable ſcope, and treat of a variety of diſeaſes, commonly contain much that may be found in older authors. This, perhaps, is ſcarcely to be avoided, when the writer's object is to give a full enumeration of all the ſymptoms, and all the remedies that have been found efficacious in each diſeaſe. Dr. P. in the truly original and ſcientific work before us, has no ſuch object; and it is his profeſſed deſign, to copy nothing from his predeceſſors.

" I have endeavoured, (ſays he) in the following work, to trace theſe ſymptoms with ſome minuteneſs and preciſion, and I truſt that the younger Profeſſors of our art, will here find materials of practical knowledge, by which they will be enabled to form a juſt and early prognoſtic of the nature and tendency of internal diſorders.

" The Obſervations, which are here ſubmitted to the attention of the public, are the reſult of ſeveral years experience: they were made at the bed-ſide of the Patient, and were faithfully recorded at the time, when the caſes were paſſing under my attention.

" As I do not mean to introduce any matter, which has not been the reſult of my own practice, or which has not ariſen from my own experience in the practice of others, the reader muſt not expect to find in this work a regular hiſtory of the abdominal diſeaſes, as they are recorded by our authors, who have collected the opinions of others on this ample and important theme.

" I muſt obſerve, however, that as I have meditated on this ſubject for a conſiderable period, and loſt no opportunity of obſervation during life, or reſearch after death, I am diſpoſed to imagine that this little volume will be found to contain ſome remarks, not altogether unworthy of attention, even to the experienced Practitioner, upon almoſt every diſorder of the Abdominal Viſcera: and he

he will, I truſt, not fail to diſcover a vein of enquiry into certain diſeaſes, which others have but ſlightly recorded, or inadequately conceived."

The following organs and parts of the abdomen, with their acute and chronic diſeaſes, are treated of in this work, viz. The peritonæum, the liver, the gall bladder, the pancreas, the ſpleen, the kidneys, the ſtomach, and the inteſtines.

In treating of inflammation of the liver, our author is naturally led to the ſubject of venæſection, and after ſeveral important obſervations on the appearance of the blood drawn, he adds:

"Phyſicians have been ſtruck, at all times, with the effect produced by taking the blood from a large orifice in inflammatory diſeaſes, and it is certainly a matter which cannot be too ſtrongly urged as an indiſpenſable point in practice: eſpecially as the generality of writers do not ſeem to have inſtituted any defined plan to regulate and ſecure the effectual performance of this important operation. I wiſh, therefore, to preſs in the ſtrongeſt terms the abſolute neceſſity of attending to that circumſtance, which the following obſervations may perhaps tend to elucidate.

"It is true, that from a *ſmall* orifice an equal quantity of blood may be taken as from a *large* one, but the *time* of its flowing is ſo long, that the topical inflammation, which demands for its relief a ſudden effect upon the ſyſtem, is not in the leaſt influenced by it, though the general ſtrength is much weakened, which is an occurrence of all others to be avoided in a diſeaſe, that requires repeated evacuations.

"As I conſider this matter of great conſequence, I ſhall endeavour to point out a method, by which a plan of a more defined nature than that hitherto adopted may be given for drawing blood in inflammatory diſeaſes.

"At preſent we are contented to order, that the blood ſhould be taken from a large or from a ſmall orifice, than which nothing ſurely can be more vague or undefined. The plan, which I propoſe, refers to the *length of time* in taking the blood, which may be meaſured, and not to the ſize of the orifice, which can not.

"I find from numerous experiments, made at my deſire for this purpoſe by different ſurgeons, that when the orifice is ſuch as to permit eight ounces of blood to flow in three minutes, that then, a patient under acute inflammation will receive every benefit which is expected from the remedy. If it flows in a longer time he will receive leſs benefit, and under certain circumſtances no benefit at all, or even an abſolute injury.

"I can ſuppoſe a caſe of peripneumony, where a patient ſhall have juſt general ſtrength enough to carry on reſpiration by the aſſiſtance of the voluntary muſcles, and that eight ounces of blood ſhall be taken from a very ſmall orifice, by which the change will be ſo gradual, in conſequence of the time required for the blood to flow, that no alteration whatever will be made in the diſeaſe, but yet the general ſtrength ſhall be ſo diminiſhed, that death will enſue. On the other hand, had the ſame quantity of blood been taken

taken by a large orifice, that *then* the difeafe would have felt the remedy, and refpiration would have gone on with lefs exertion of the remaining general ftrength, in confequence of the lungs being relieved by this fudden depletion.

"The great confequence, therefore, attached to the mode in which blood is drawn, (as on this alone life or death may often depend) imperioufly demands of every phyfician to imprefs upon the mind of his patient the importance of the operation, and the abfolute neceffity of having it performed by a perfon fully fkilled in his profeffion.

"I fhould not omit to mention, that there may now and then occur a cafe, where the vein may not only be particularly fmall, but likewife be deep feated, and covered with fat. Here, altho' the orifice may be fufficiently large, yet a portion of fat will obtrude, fo as to interrupt the ftream of blood. I would in fuch cafe recommend the furgeon to dilate the external orifice, or even make a frefh orifice, rather than perfift in his endeavours to obtain the quantity required in this gradual way."

We have given thefe obfervations in the Author's own words, without abridgment, becaufe they appear to us of confiderable importance in practice.

(To be concluded in our next.)

An Account of the Practice of one of the Physicians of the Westminster General Dispensary, and of the Western Dispensary; from March 20 to April 20, 1806.

Acute Diseases.	
Continued Fever	2
Scarlet Fever and Sore Throat	3
Measles	1
Peripneumony	1
Hæmoptoe	1
Catarrh	10
Acute Rheumatism	4
Colic	2
Dysentery	1

Chronic Diseases.	
Cough and Dyspnœa	38
Pulmonary Consumption	2
Scrophula	2
Rheumatism	5
Lumbago	6
Cephalalgia and Vertigo	5
Gastrodynia	6
Enterodynia	3
Diarrhœa	5
Asthenia	8
Dropsy	2
Pyrosis	1
Jaundice	3
Vomiting	2
Abscess of the Liver	1
Dysphagia	1
Dysuria	1
Deafness	1
Psora	5
Syphilitic Eruption	2

Periodical Diseases.	
Quotidian	3
Hemicranium	1
Tic Douloureux	1

Dis-

DISEASES OF WOMEN AND CHILDREN.	Hysteria - - - - - - 2
	Hooping Cough - - - 1
Chlorosis & Amenorrhœa 4	Scarlatina - - - - - - 2
Menorrhagia - - - - 3	Small-pox - - - - - - 1
Puerperal Fever - - - 1	Pyrexia from teething - 1
Inflammation of the Breast 1	Peripneumony - - - 1
Abortus - - - - - - 1	Catarrh - - - - - - 3
Gravid Uterus with Pyrexia 2	Scrophula - - - - - - 1
	Worms - - - - - - - 2
Prolapsus Uteri - - - 1	Diarrhœa - - - - - - 2
Leucorrhœa - - - - 3	Scald Head - - - - - 1

Since the last Report, catarrhal fever, cough and dyspnœa, have been very general throughout the metropolis and its vicinage. In some instances the symptoms were obstinate, and many of the patients were affected with pain in the chest and side, which only yielded to blistering. Acute rheumatism has also been a frequent and troublesome complaint; the difficulty of cure has, in some cases, been augmented by the accession of cough and affections of the chest. In this town, bleeding is now seldom used in the cure of acute rheumatism; for though present relief be obtained by the use of the lancet, the symptoms generally return, and the patient is less able to combat with their violence; if, however, blood must be drawn, topical bleeding with leeches offers the most effectual means of reducing the inflammation, without materially affecting the strength of the patient. In Scotland a very common and a very successful mode of practice, is to place the patient between blankets, give frequent doses of pulvis ipecacuanhæ compositus, with copious diluting beverage, and thus keep up a profuse perspiration for several hours, when a complete cure is often effected. This practice is less beneficial in London, where the atmosphere is less pure, and the habits of the people less conducive to health; I have tried it in several cases, while in others I have used bark, sometimes with antimonials, and sometimes with acids, occasionally giving opium; the result of about twenty cases, is in favour of the latter mode of practice. Sleep is often procured, a remission of all the symptoms obtained, and the cure materially hastened by the judicious exhibition of opium.

During the greatest part of the month, the wind was easterly; and with the exception of three or four fine days, the weather has been cold, and some snow has fallen. To this severe commencement of spring, after a wet and

and mild winter, may safely be attributed the cause of the numerous pulmonic and catarrhal affections, noticed in this month's report.

Two of the cases of sore throat were attended with fever and scarlet eruption; one of them occurred in a private patient. She had for a month previous to the attack, laboured under great anxiety of mind and depression of spirits, had confined herself to the house, and almost to one apartment, when she began to complain of sore throat and fever. On the third day the fever was disposed to be malignant; the skin was dry, the heat very little increased beyond the natural standard, the tongue furred, with a quick feeble pulse, and great prostration of strength. She complained of much pain in her head and bones, great difficulty of swallowing, and was very faint and unwilling to use the least exertion; in consequence of which, the throat was not inspected till the following day, when a deep ulcer was observed on the right tonsil. Large scarlet blotches now appeared on the arms and about the breasts; and though not delirious, she was extremely low, and averse from taking nourishment or medicine. The consequence of such conduct being insisted upon, she consented to take the medicines. A blister had been applied round the throat, and bark was now given in decoction and compound tincture, as freely as the stomach would bear; and the bowels were gently opened with calomel and rhubarb. On the eighth day, she began to mend, desquamation of the cuticle took place, and she soon had only to contend with debility. The complaint was not traced to contagion; no person in the family was affected by it, and she had not previously been out of doors for several weeks. One of the other cases, was in a child aged ten years, and differed from the above, in being attended by delirium, and the scarlet eruption was more generally diffused over the surface of the skin. The treatment was nearly the same, and the termination equally favourable. In both cases, a small dose of calomel and rhubarb given at night, was evidently beneficial, the tongue, in the morning, being much cleaner, and the patient more cheerful and alert.

SAMUEL FOTHERGILL.

Southampton Street, Strand,
April 22, 1806.

MEDICAL

MEDICAL AND PHYSICAL INTELLIGENCE.

Bishop MADISON, of America, has been enabled to ascertain that the Mammoth, or American Elephant, was an herbivorous animal.—In digging a well in Wythecounty, Virginia, after penetrating about five feet and a half from the surface, the labourers struck against the stomach of a Mammoth, the contents of which were in a state of perfect preservation, consisting of half masticated reeds, twigs, and grass, or leaves. The bishop adds, "there could be no deception; the substances were designated by obvious characters which could not be mistaken, and of which every one could judge; besides, the bones of the animal lay around, and added a silent, but sure confirmation." In addition to this fact it may be deserving of notice, that Mr. Francis Nevil, in his account of the elephantine teeth that were discovered in the north of Ireland, early in the eighteenth century, has mentioned some facts relative to the long preservation of vegetable matters, which seem worthy of our notice in this place: and the more so, as this gentleman's paper seems not to have excited any attention among the modern writers on the exuviæ of animals found in countries in which the living animals themselves are no longer seen. Some extravagant conjectures are mixed with Mr. Nevil's account: but these do not, in the least, invalidate the truth of what he says, relative to the bed upon which the Irish elephant was laid. "The place (says he) where this monster lay, was thus prepared, which makes me believe it had been buried, or that it had lain there since the deluge. It was about four feet under ground, with a little rising above the superficies of the earth, which was a plain under the foot of a hill, and about thirty yards from the brook, or thereabout. The bed whereon it lay had been laid with fern, with that sort of rushes here called sprits, and with bushes intermixed. Under this was a stiff blue clay, on which the teeth and bones were found: above this was, first, a mixture of yellow clay and sand, much of the same colour; under that a fine white sandy clay, which was next to the bed; the bed was for the most part a foot thick, and in some places thicker, with a moisture clear through it; it lay sad and close, and cut much like turf, and would divide into flakes, thicker or thinner as you would wish; and in every layer the seed of the rushes was as fresh as if new pulled, so that it was in the height of seed-time that those bones were laid there. The branches of the fern in every lay as we opened them, were very distinguishable, as were the seeds of the rushes and the tops of the boughs. The whole matter smelt very sour as it was dug, and tracing it I found it 34 feet long and about 20 or 22 feet broad."—"I forgot to mention that there was a great many nut-shells found about the bed, perhaps those might have been on the bushes which composed part of the bed."

Some

Some members of the Galvanic Society at Paris, have ascertained that the galvanic action is augmented, 1. When the pile is exposed to a high temperature; 2. When it is plunged into flame, or in oxygen gas, or carbonic acid gas; 3. That the effects of the pile are not transmissible in vacuo, or that they are then scarcely perceptible, even by means of a condenser. It has been also ascertained that galvanism cannot be transmitted through smoke.

M. HERMAN, of Berlin, has examined the properties of different substances employed as Galvanic conductors, and has divided them into insulated bodies, into perfect and imperfect conductors, and into uni-polar and bi-polar bodies, according as their conductible property manifests itself at both, or only at one of the extremities of the pile.

Mr. HORNE has laid before the Royal Society of London, a paper describing a particular affection of the prostrate gland.— This disease, which occasioned so much pain, has hitherto been deemed irremediable; but it is now hoped, the cause having been discovered, that this physiological discovery may prove of incalculable advantage in relieving the sufferings of patients supposed to be labouring under the effects of calculi, and other urinary disorders.

On Wednesday the 16th of April, the Royal College of Surgeons adjudged the Jacksonian Prize for 1805, to John Hyslop, Esq. surgeon, Fenchurch-street, for the best Dissertation on "Injuries of the Head from external Violence."

Mr. BROOKES, in Blenheim Street, will commence his summer course of Lectures on Anatomy, Physiology, and Surgery, on Saturday, June 7, at two o'clock in the afternoon, and will be continued on Monday the 9th, and every subsequent morning at seven, Sunday excepted. The Dissecting Rooms will be opened at five in the morning.

Mr. CARPUE will commence his Lectures on Anatomy and Surgery early in June. Particulars may be known by applying to Mr. Carpue, No. 50, Dean Street, Soho.

Dr. HOOPER will commence a Summer Course of Lectures on the Theory and Practice of Physic, Materia Medica, and Pharmaceutical Chemistry, on Monday the 26th of May, at his Lecture Room, No. 26, Cork Street, Burlington Gardens, at half past eight o'clock in the morning. A Prospectus, with particulars, may be had at his house, No. 13, Holles Street, Cavendish Square.

Mr. JOHN TAUNTON, on the 24th of May, will commence the summer course of his Lectures on Anatomy, Physiology, Pathology, and Surgery.

Dr. REID will deliver the introductory Lecture to his Summer Course, on the Theory and Practice of Medicine, on Friday the 30th of May, at eight o'clock in the evening, at his house, Grenville-street, Brunswick-square; at which place the subsequent Lectures

tures will also be delivered, at ten o'clock in the morning, every Monday, Wednesday, and Friday, until the conclusion of the Course.

At the beginning of this month will be published, a new edition (with some additions) of Dr. Clarke's Essays on the General Management of Pregnancy and Labor, and on the Inflammatory and Febrile Diseases of Lying-in Women, under the title of Essays on Subjects connected with the Practice of Midwifery, and the Diseases of Women and Children.

Dr. WILLAN has in the press a work on the Cow-pox, and on its varieties and anomalies, to be illustrated with a series of highly finished and accurate engravings, in the manner of his work on Cutaneous Diseases.

Dr. E. G. CLARKE intends to publish, as soon as possible, Observations on the Diseases of the Army.

Mr. HUTCHINSON, of Southwell, is preparing for the press, An Essay on the Narcotic Powers of the Tartrite of Antimony introduced into the Syftem by the cutaneous Absorbents. This Essay is expected to be publifhed in July or August.

LIST OF NEW MEDICAL PUBLICATIONS.

The Vaccine Contest; or, "Mild Humanity, Reason, Religion, and Truth, against fierce unfeeling Ferocity, overbearing Insolence, mortified Pride, false Faith, and Desperation;" being an exact Outline of the Arguments and interesting Facts adduced by the principal Combatants on both Sides, respecting Cow-pox Inoculation; including a late official Report on this Subject, by the Medical Council of the Royal Jennerian Society. By William Blair, 2s. 6d.

A Treatise on Epilepsy, and the Use of the Viscus Quercinus, or Misletoe of the Oak, in the Cure of that Disease. By H. Frazier, M. D. &c. 2s. 6d. 8vo. sewed.

Surgical Observations on Health. By Mr. Abernethy. Part 2d. 6s. 8vo. bds.

The Medical Observer; containing an impartial Account of advertised Nostrums, their Composition, and the dangerous Consequences that must inevitably arise from their indiscriminate Use, particularly such as are advertised under fictitious Names. To which are added, a few Specimens of the profound Erudition of a Member of this learned Body (being an original Correspondence); with Observations addressed to the Right Honourables, the Right Reverends, and Commoners, who have permitted their Names to appear in the public Prints, in Support of a Practice, which is calculated to destroy many thousand Members of the British Community annually. 2s. 6d.

Observations on Abortion; containing an Account of the Manner in which it is accomplished, the Causes which produced it, and the Method of preventing or treating it. By John Burns. 4s. 6d. 8vo. bds.

TO CORRESPONDENTS.

Communications are received from Dr. Bins, Dr. Wood, Mr. Blegborough, Mr. Dashwood, Dr. Walker, Mr. Young, Dr. Kinglake, and Dr. Bellamy.